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A Letter from Mr. BLAIR *to Dr.* BRADLEY, *on Cancerous Hydatids.*

DEAR SIR,

IF the following Caſe ſhould appear to you ſufficiently intereſting to lay before the public, and tends in any degree to illuſtrate the opinion of Dr. Adams on the nature of cancerous complaints, it is at your entire diſpoſal. But, in communicating a particular fact, I wiſh it to be underſtood, that I do not pledge myſelf to ſupport a ſyſtem or a theory: nor do I think this to have been the deſign of Dr. Adams, when he publiſhed his Obſervations on Morbid Poiſons;" for, in the chapter "OF CARCINOMA AND OTHER LOCAL DISEASES USUALLY CALLED CANCER," he expreſsly ſays, "I am only propoſing enquiries, without venturing to form theories."

So far as I can collect from his work on Morbid Poiſons, it appears, that the exiſtence of cyſts or hydatids, poſſeſſed of a life independent of the ſubject in which they grow, is regarded by the author as conſtituting the eſſential character of the "true carcinoma." By the term carcinoma he intends to deſignate all thoſe affections which (properly ſpeaking) have been denominated ſcirrhus and cancer; and although he "knows of nothing that can diſcriminate early carcinoma from other tumours," this diſeaſe is ſuppoſed always to aſſume the animalcular form.

The chief ſubject of controverſy, I think, will be reſpecting the *genuineneſs* of the complaint, and the diſtinction of *carcinomatous hydatids.* In cancerous caſes, where the hydatid is found, perhaps no queſtion will ariſe; but, where this animal is wanting, may it not be urged that the diſorder was not really cancerous? "In all the cancerous breaſts, teſticles, and tumours, which I have examined," ſays a very recent writer, "I never ſaw any thing which could be conſidered diſtinctly as a hydatid." (Burns' Diſſert. on Inflam. Vol. II.) Until lately I could have ſaid the ſame; but the caſe I have juſt met with, will

will make me more circumſpect in my examination of cancerous ſubſtances.

On the 15th of May 1801, Mr. Chambers, a ſurgeon at Deal, ſent me a letter by one of the pilots of that town, requeſting my opinion on the nature of a ſwelled teſticle, under which the bearer had laboured a conſiderable time. The tumour ſo nearly reſembled a true ſcirrhus, that Mr. Chambers wiſhed the patient to ſuffer its extirpation. My opinion entirely coincided with his; and as the ſpermatic chord was becoming much enlarged and thickened, my advice was that the teſtis ſhould be removed immediately. I obſerved, however, that the external ſurface of the tumour was not ſo irregular as ſcirrhous teſticles generally are; but its extreme hardneſs and other ſymptoms peculiar to ſcirrhus were ſufficiently well marked to juſtify the operation.

On the 5th of June, Mr. Chambers wrote me, that he had extirpated the teſtis on the preceding day. He alſo did me the favour to ſend above half the amputated part, the other half having been preſerved by the patient himſelf. I had the pleaſure to find that the moſt intereſting maſs of the diſeaſe was included in the portion I received: for the greater part of its ſubſtance, eſpecially the ſuperior part, was granulated and ſemi-cartilaginous in its texture, as if interſected with oſſeous filaments of a light brown appearance; but in the centre of the inferior portion was a livid, pulpy, and irregularly fibrous maſs, nearly of the ſize of a walnut, almoſt ſurrounded by tranſparent ſpherical veſicular bodies, moſt of which were not larger than ſmall peas, perfectly detached from the adjacent ſubſtance, and from each other, and, when punctured, diſcharging a pellucid fluid with conſiderable force.

I ſtate the appearances as I found them, and, upon the whole, cannot but imagine the above caſe to be one of thoſe which Dr. Adams would adduce in ſupport of his ſentiments. Of this, however, I leave you to judge. I certainly believe it was a genuine carcinomatous affection, and that the cyſts were ſuch as many perſons would denominate hydatids.

I remain, &c.

W. BLAIR.

Great Ruſſel Street, Bloomſbury,
July 20, 1801.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

I Have taken the liberty of ſending you an account of the caſes admitted at the Bath City Diſpenſary, in the courſe of the laſt month, if you think it worth a place in your valuable Publication. I am, &c.

Bath, Aug. 8, 1801. W. WHITE.

From the following ſtatement, it will be found that the Synochus Bilioſa is the moſt prevalent diſeaſe we have at preſent in this city; and as it has been attended with ſome peculiar circumſtances hitherto unnoticed, I hope to have in my power ſoon to communicate to the public a ſhort hiſtory of it.

Account of Patients admitted from July 1, *to Aug.* 1, 1801.

Angina	2	Pulmonary Compl.	1
Aſthma	1	Prolapſus Ani	1
Anaſarca	1	——— Uteri	1
Catarrhus Febrilis	1	Pſora	7
Chloroſis	2	Rheumatiſm Acute	10
Contuſion	1	Rachitis	1
Eruptiones	3	Synochus Bilioſa	38
Febris Simplex	4	——— Quotidiana	1
——— Quotidiana	1	——— Pleuritica	5
——— Puerperalis	1	——— Enteritica	3
Gaſtrodynia	1	——— Gaſtritica	4
Hydrocephalus	1	——— Rheumatica	4
Hernia	2	——— Cholerica	2
Hæmoptyſis	1	——— C. Hemiplegia	1
Hæmatemeſis	1	——— Nephritica	1
Herpes	2	Syphilis	4
Icterus	1	Sprain	1
Lepra	3	Suppuratio	1
Menorrhagia	2	Strictures in Urethra	1
Nephritic Compl.	1	Tumor in the Groin	1
Ophthalmia	1	Tuſſis	2
Pleuritis	1	Tinea Capitis	1
Pertuſſis	2	Varicoſe Ulcers	10
Phthiſis	1	Variola Diſcreta	1

On my firſt viſiting the perſon attacked with Hemiplegia, I was not clear whether it was an original or ſymptomatic affection, but was afterwards convinced it was the latter, and the complaint a variety of the Synochus Bilioſa. The ſubject of

 it,

it, (a man aged about 40) was ſeized in the morning with giddineſs, dimneſs of ſight, and head-ach; ſucceeded by the loſs of ſpeech, and of the uſe of his right ſide. I did not ſee the patient till very late in the evening; his pulſe was full, ſtrong, and frequent; he could not ſpeak, but appeared under great anxiety. He had vomited ſome green bile. Before I ſaw him, twelve ounces of blood had been taken from his arm, which was highly inflamed; and as his pulſe indicated the neceſſity of repeating the operation, I took away eight ounces more; after which ten grains of calomel were preſcribed, and a ſolution of neutral ſalts to be taken every four hours. Next morning his bowels were moved, and a great quantity of dark bile was evacuated; after which he could move his fingers a little, and the day following perfectly recovered his ſpeech, and the uſe of his arm and leg.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

IN the catalogue of diſeaſes which generally prevail and afflict the moſt indigent claſs of this city are thoſe of the infectious kind, particularly Putrid and Typhus Fevers. The extreme diſtreſs under which the poor have lately laboured, has not only been conducive to the propagation of this diſeaſe, but alſo to its malignity; and at the ſame time it has operated as a great obſtacle to the cure. However, I flatter myſelf that I have been as fortunate in the treatment of them as I could expect; and a great number having been placed under my care, I have had a ſufficient opportunity of confirming the general efficacy of the medicine I have employed.

The uſe of yeaſt, whenever I had a fair opportunity of trying it, proved very ſucceſsful; but I have experienced many diſappointments in general with it, owing to the obſtinate prejudice of the indigent againſt any thing they ſuppoſe themſelves familiar with, and capable of judging about. If the patients are willing to take it, the viſitors, friends, and neighbours will ſoon, without farther inveſtigation, prejudice them againſt it.

In conſequence of theſe accidental inconveniencies, I have endeavoured to adopt a treatment as cheap as eligible, and as agreeable to my ſituation; after five months trial, I found its effects to anſwer my moſt ſanguine expectations. The reſult of

of it was, in that time I admitted 211 Typhus caſes, out of which 11 died.

My treatment was as follows: In the firſt ſtate of the fever I univerſally gave an emetic; then the following mixture, ℞. Acid. muriat. ʒj. Syr. ſimp. ℥ſs. Decoct. cinchonæ flavæ ℥ij. Aq. puræ q. s. ft. miſt. capt. coch. iij. amp. 4tis horis; and in the more advanced ſtate I gave the following, ℞. Acid. muriat. ʒj. Decoct. cinchonæ flavæ ℥viij. M. capt. coch. iij. amp. 4tis. horis.

Diſpenſary Houſe, North Street,
Auguſt 10, 1801.

I am, &c.
W. D. ROLFE.

State of the Patients of the Bristol Diſpenſary, from the 1ſt of July to the 1ſt of Auguſt, 1801.

Typhus Fevers - - - -	39	Gaſtrodynia - - - - -	1
Putrid Fevers - - - - -	2	Dyſpepſia - - - - - -	1
Inflammatory Fevers - -	6	Diarrhœa - - - - - -	1
Bilious Fevers - - - - -	3	Aſthma - - - - - - -	2
Miliary Fever - - - - -	1	Eryſipelas - - - - - -	3
Intermittent Fever - - -	1	Sarcocele - - - - - -	1
Anaſarca - - - - - -	2	Scorbutic - - - - - -	1
Aſcites - - - - - - - -	1	Teneſmus - - - - - -	1
Pleuriſy - - - - - - -	1	Exanthema - - - - - -	1
Hæmorrhoids - - - - -	1	Dyſenteria - - - - - -	2
Variola - - - - - - - -	1	Rheumatiſm - - - - -	1
Podagra - - - - - - - -	2	Cardialgia - - - - - -	1
Conſumption - - - - -	5	Uterine Hæmorrhage - -	2
Ophthalmia - - - - - -	1	Scrophula - - - - - -	1

Account of Diſeaſes in an Eaſtern Diſtrict of London, from the 20th of July, to the 20th of Auguſt, 1801.

	No. of Caſes.		No. of Caſes.
ACUTE DISEASES.		Cephalalgia - - - - - -	5
Typhus - - - - - - -	28	Paralyſis - - - - - - -	3
Hepatitis - - - - - - -	1	Vomitus - - - - - - -	3
Acute Rheumatiſm - -	1	Diarrhœa - - - - - -	8
CHRONIC DISEASES.		Dyſpepſia - - - - - -	8
Tuſſis - - - - - - - -	10	Chloroſis - - - - - - -	4
Tuſſis et Dyſpnœa - -	7	Menorrhagia - - - - -	4
Pleurodyne - - - - - -	3	Aſthenia - - - - - - -	4
Phthiſis Pulmonalis - -	2	Scrophula - - - - - -	3
Hydrothorax - - - - -	2	Hæmorrhois - - - - -	2
Anaſarca - - - - - - -	3	Hypochondriaſis - - -	3
Aſcites - - - - - - - -	1	Pſora - - - - - - - - -	4

Herpes

Herpes - - - - - -	7
Lepra - - - - - - -	1
Rheumatiſmus Chronicus	13
PUERPERAL DISEASES.	
Peritonitis - - - - - -	1
Maſtodynia - - - - -	2
Menorrhagia Lochialis -	3
INFANTILE DISEASES.	
Vermes - - - - - -	2
Herpes - - - - - -	5
Diarrhœa - - - - -	4

Whilſt we are under the neceſſity of again adverting to a ſubject, which we have long been obliged to conſider,—the prevalence of the typhoid contagion—we are, at the ſame time, happy to announce, that the ſymptoms of the diſeaſe are much leſs formidable than they have heretofore been, and that a fatal termination takes place much leſs frequently. The number of patients affected by it has indeed been rather increaſed, and it continues to propagate itſelf very extenſively; but thoſe affections of the brain, which formerly conſtituted ſo prominent a feature of the diſeaſe, occur leſs frequently, and appear under a milder form.

The ſymptoms of Angina alſo, which in the laſt Report were mentioned as accompanying the diſeaſe, have diſappeared. The diſeaſe therefore now appears under a form in which we have heretofore been accuſtomed to view it.

Complaints of the ſtomach and bowels have been very frequent. Diarrhœas have in ſeveral inſtances been troubleſome, and have continued for a conſiderable time; but they have not been attended with any unpleaſant conſequences, unleſs when they have been injudiciouſly checked by aſtringents; or when, under the idea of ſupporting the ſtrength of the patient during the increaſed evacuation, recourſe has been had to ſome warm cordial. Upon a premature check to this evacuation we often hear of pains in the ſtomach and bowels, of which the patient did not before complain, and the removal of which is beſt accompliſhed by inviting the return of that diſcharge which Nature had inſtituted with a view to relieve the conſtitution.

Diſeaſes admitted under the Care of the Phyſicians of the Weſtminſter Hoſpital, from the 25th of June to the 24th of August, 1801.

Continued Fever - - -	24
Pleuritis - - - - - - -	2
Enteritis - - - - - - -	2
Eryſipelas - - - - - -	1
Amenorrhœa - - - -	2
Anaſarca - - - - - - -	8
Aſcites - - - - - - -	2
Aſthenia - - - - - - -	1
Aſthma - - - - - -	1
Catarrh - - - - - -	1
Cephalæa - - - - -	1
Cholera - - - - - -	3
Colic - - - - - -	3
Diarrhœa - - - - -	7
Dyſpepſia - - - - -	2
Dyſpnœa - - - - -	5

Dyſuria

Dyſuria	3	Obſtipatio	1
Enterodynia	5	Paralyſis	3
Gaſtrodynia	3	Pertuſſis	1
Hæmoptoe	1	Pleurodyne	1
Hydrocephalus	1	Rheumatiſm	10
Hyſteria	2	Struma	3
Icterus	1	Syphilis	1
Impetigo	2	Tabes	1
Leucorrhœa	1	Tuſſis	10
Lithiaſis	1	Vertigo	3
Lumbago	2	Vomitus	2
Menorrhagia	3		

On the Nature of Chymus and Chylus.

THE nature of the *Chymus* and *Chylus*, and the changes which the firſt undergoes in the *inteſtina tenuia*, have not yet been ſufficiently examined, however intereſting a more accurate chemical analyſis of both ſubſtances may be for Phyſiologiſts. We are, therefore, much indebted to ſome able chemiſts and phyſicians, who have endeavoured to throw new light upon this ſubject by ſeveral chemical operations, the reſult of which, we ſhall communicate in the following lines. It is known from the experiments of HUNTER and SPALANZANI, that the ſtomach is endued with the faculty of making the milk and albuminous matter coagulate in a very ſhort ſpace of time, a phenomenon which is generally derived from an acid in the ſtomach, though ſeemingly contradictory to the later experiments of PARMENTIER and DEYEUX, (*Experiences et Obs. sur les differentes Especes de Lait. Straſb. 7. p.* 360), according to which the interior membrane of the ſtomach retained the property of coagulating the milk, even after having previouſly been treated with alkalis. The chymus of animals, however, ſhows moſt evident traces of an acid, particularly in the carnivorous tribe, which ſeems not to be very volatile, becauſe, being expoſed to the air, and boiled with water, it by no means loſes the property of reddening blue vegetable tinctures. It neither coagulates in the air nor by the fire, and after being inſpiſſated, it may be again diſſolved in water. Mr. SPALANZANI imagines, that the acid of the chymus originates from the remainders of the food, which opinion, however, is inconſiſtent with later experiments, according to which, the ſuccus gaſtricus and inferior membrane of the ſtomach have always been found

found to be of an acid nature in animals that were purpofely fuffered to hunger, and being killed fome time after, the moft inconteftable traces of an acid were difcovered in their ftomachs.

The action of the fuccus gaftricus feems to confift, according to Profeffor AUTENRITH of Tübingen, in an oxydation, through means of which the albuminous fubftances of the ingefta coagulate, and are again diffolved by the acid and falts of the fuccus gaftricus. The juice that is found in the duodenum and inteftina tenuia contains particles coagulating in the air, and poffeffes at the fame time very little or no acidity, which is likewife not to be difcovered in the contents of the inteftina craffa. Chymus, and the contents of the inteftina tenuia in herbivorous and carnivorous animals differ, therefore, by the latter being not acid, and by their coagulating in fire and in the air. The contents of the inteftina craffa agree with chymus by not coagulating, though they differ from it by fhewing not the leaft traces of an acid. It has, however, been remarked by VAUQUELIN, that the excrements of men contain an acid fimilar to the acetous acid. (FOURCROY *Syftème des Connaisfances Chimiques, T. X. p.* 70. *A.* 312.)

Chyle has the appearance of a thin milk, becomes whiter in the air, and coagulates only in part; it is likwife deprived of all oily particles. It differs from lympha by being milky.

We fhall now enter upon the queftion, how the chymus is converted into chyle, which proceeding takes place in the duodenum, and is thought to be effected by the bile; in order to afcertain which, the chymus of carnivorous and herbivorous animals was mixed with bile, from which a precipitation was produced, and a white cuticule arofe on the furface, a phenomenon that even takes place when the chyle and bile of different animals are mixed together. No precipitation is occafioned by the gall in the contents of the inteftina craffa. Although this white precipitate feems to have been pre-exiftent in the chyle, yet one may fuppofe it to originate from the albuminous part of the gall, which is feparated by the acid of the chymus, becaufe a fimilar precipitation enfued, on adding vinegar to pure gall. In order to afcertain this circumftance, 27 grains of the chyle of a cat were mixed with 14 gr. of bile; the precipitate thus obtained, weighed about 3 ¼ gr. To 18 gr. of bile taken from the fame cat, 27 gr. of vinegar were added, by which a precipitate was produced, weighing only 2 ⅔ gr. when dry. 4 fcruples of chymus of a cat precipitated in *one* drachm of gall about 9 gr. whereas 4 fcruples of alkohol only precipitated 3 ¾ gr. from the fame dofes of gall. It feems to refult from thefe experiments, that the above mentioned precipitation is

is owing both to the albuminous parts of the gall, as well as to the chymus.

For explaining the manner in which the chymus is changed into chyle, it may be adopted, that it proceeds from an absorption of oxygen, and the procefs of digeftion is accordingly performed in the following fucceffion. The albuminous parts of the food coagulate in the ftomach by an oxygenation, and are again diffolved by the acid of the fuccus gaftricus, but partly changed into a milky emulfion. The whole mafs is now deprived of its oxygen by the addition of the gall, whereby the lymphatic parts of the gall and of the food coagulate in fmall particles, which are again changed into a mucilaginous mafs, by a greater defoxydation, which poffeffes the property of coagulating on contact with the atmofphere, and after being abforbed by the lymphatic veffels, obtains the name of chyle.

According to the experiments of Dr. REUSS and EMMERT, the chyle has more analogy with blood than with milk, on which account it rather deferves the appellation of white blood than of milky juice. It neither contains oily particles nor faccharum lactis, the characteriftic conftituents of milk. We may alfo conjecture, that the phofphat of the oxyd of iron, which, according to the interefting difcovery of FOURCROY and VAUQUELIN, is the colouring matter of the blood in the ftate of fuperoxydation, does pre-exift in the chyle in form of a white phofphat. (CH. L. WERNER, *Experimenta circa modum quo Chymus in Chylum mutatur, in Animalibus instituta præside* J. H. F. AUTENRITH, Prof. Tubingenfi.)

Observations on Simple Fractures where the Union fails. *By* A. CARLISLE, Efq. Surgeon to the Weftminfter Hofpital, &c. &c.

To Dr. BATTY.

DEAR SIR,

ONE of the unpleafant accidents which occafionally happen in the treatment of fimple Fractures of the Cylindrical Bones, is the failure of union. It may poffibly have occurred to fome of your readers to note particular circumftances either in the general conftitution, the mode of treatment in fuch cafes, or peculiarities in the manner of the fracture. As I have not met with any fatisfactory reafons for the defect of offific union in thofe occafional inftances, I beg leave to ftate the following hiftories,

hiſtories, which may perhaps induce gentlemen of greater experience, and better knowledge, to clear up this obſcurity. The following three caſes were the ſubjects of memorandums at the time they preſented themſelves, and which record is as follows.

A ſailor, about forty-five years of age, on board one of His Majeſty's ſhips of the line on the Jamaica ſtation, had his thigh fractured by a fall, and ſoon afterwards returned home with a fleet. After fifteen months the broken thigh had not acquired any firmneſs, but it was eaſily moved, as if a joint was formed at the fracture. The ends of the broken bone had paſſed each other two inches. The man was athletic, of dark complexion, chewed much tobacco, and drank ſpirituous liquors whenever he could obtain them. By his own account, the treatment of his limb, both as to bandages and reſt, had been according to the common routine. This man was induced to undergo a painful operation; both ends of the fractured femur were ſawed off to the extent of an inch and a half from each, and although the limb was carefully attended to, yet the union did not obtain, and he remained to the time of his death with a flexible joint in the middle of his thigh.

A young athletic man, between twenty and thirty years of age, by trade a houſe carpenter, had a ſimple fracture of the os brachii about its middle; the bandages and ſplints in common uſe were applied, and it was concluded as a matter of courſe that his limb would become firm in the ordinary time. At the end of ſix months I obſerved him ſtill uſing the ſling, and carrying his fore-arm and hand like a dead limb. On removing the bandages the fracture had not united, and motions of the limb did not occaſion pain in the part. This man was a patient in the Weſtminſter Hoſpital, and had been enjoined low diet and extenſive evacuations, which over anxiety on his part had carried to great extent. His limb had always hung low in the ſling, the fore-arm and hand were conſtantly œdematous, and the upper arm had ſtretched full two inches in length, ſo that the ends of the fracture were ſeparated to this diſtance. I ſaw him twelve months afterwards with a diſunited bracheum, having tried a variety of methods to excite union, but all unſucceſsfully.

A ſoldier, between thirty and forty years old, had his tibia broken and fibula diſlocated; he had been treated in a military hoſpital, and the lowering plan carried to its full extent. After ten months the broken bone was looſe, although a piece of ſplinter appeared to be intervening; it is very probable that this man's limb became firmer, but I have never heard of his fate.

Although caſes of defective oſſification after ſimple fractures are

are uncommon, yet when they do happen it is very diftreffing to both the patient and practitioner. Nor is there any decided inference to be drawn from thefe cafes, as to the certain caufe of fuch misfortunes.

The failor's thigh did not fail, becaufe the broken ends were pufhed beyond each other; for this commonly happens to the fame extent, and yet the fracture unites; and the refult of his operation fhewed fomething like a conftitutional deficiency in the offific procefs.

The carpenter's arm muft have been in a favourable pofition for union during the earlier period, for it was kept remarkably ftraight.

The foldier had experienced no other treatment or fymptom beyond what may be confidered as common routine.

In all thefe men the vafcular fyftem appeared fluggifh, their pulfes were flow, and the characteriftics of inflammatory difpofition were wanting. Perhaps, in fuch conftitutions where the offific union has once failed, or where the fluggifh, inactive difpofition of the vafcular fyftem is well marked, it may be prudent to watch the natural progrefs of inflammatory fymptoms; to moderate them when actually prefent, *rather than to anticipate their appearance*; and after the thirtieth day, to commence a more generous regimen.

It has not occurred to me to make obfervations on a number of fimilar cafes which I have feen, and which, perhaps, other practitioners may have noticed, with a view to an explanation of this phenomenon. I am,

Dear Sir,

Yours, &c.

Soho Square, Aug. 26, 1801. A. CARLISLE.

The importance of Eudiometrical Obfervations and Experiments, as leading to an improvement of the means of preventing difeafe, has long been acknowledged: In continuation, therefore, of the fubject commenced at page 10, of the prefent volume, we extract from the American Philofophical Tranfactions, No. XXXII. the following

Experiments and Obfervations on Land and Sea Air. By ADAM SEYBERT, M. D.

AN endeavour to add any facts or obfervations to a branch of knowledge, which has been treated of by many of the moft enlightened philofophers of the prefent century, may be

 deemed

deemed a hazardous attempt. But although we have many accounts of eudiometrical experiments by Prieſtley, Fontana, Ingenhouſz, and others, the ſubject is not exhauſted, and an extenſive field continues open for him who wiſhes to engage in this intricate branch of pneumatic philoſophy.

The purity of the air is not intereſting to us merely as an object of curioſity, but demands our attention as phyſicians and philoſophers. In proportion to the number of aſcertained facts, the certainty of inference is increaſed. The ſhort life of any one individual, together with his local ſituation, will prevent him from completing this department of ſcience. It is merely from repeated experiments made under different circumſtances, that we can expect to arrive at truth. The more we multiply facts, the more decided may we be in our concluſions. Such are the reflections, which induced me to engage in a ſeries of experiments, which ſhall be related in the following pages.

Our atmoſphere having been ſo ſucceſsfully analyzed by the celebrated Lavoiſier, and being found to conſiſt of fluids poſſeſſing very different and oppoſite qualities, chemiſts ſoon began to enquire whether its ingredients might not be in various proportions in different ſituations; and, particularly, whether it differed in point of purity in different ſituations on land and on the ocean.

Moſt of the experiments, of which we have an account, were made on land: The Memoir of Dr. Ingenhouſz, publiſhed in the 70th volume of the Philoſophical Tranſactions, is the only eſſay I have ſeen containing experiments made at ſea; but his traverſe was ſo ſhort, that he had not an opportunity of examining the air in different latitudes. He, however, is of opinion, that ſea air is, cæteris paribus, purer than land air; but he appears to have found ſome ſeeming contradictions of his general inference. He ſays, page 364, that air taken from the middle of the channel was of an inferior quality to that at the mouth of the Thames; and that air near the ſea ſhore at Oſtend, was nearly as good as that at the mouth of that river. Although we may, to a great degree, adopt his ſentiments, nevertheleſs, I think it probable, that this increaſed purity does not entirely depend upon the ocean; for I have found the air over the bays of Cheſapeak and Delaware of the ſame degree of purity with the atmoſphere of the ocean. And hence I am inclined to think, that the air over a large body of water, is always purer, cæteris paribus, than that of the adjoining land, owing perhaps to a decompoſition, which the water may ſuffer from the action of the ſun's rays; and this may likewiſe be aiſted by its alſo abſorbing many foreign matters which

which on land are more or lefs intimately mixed with the air in a mechanical way. This opinion is confirmed by Dr. White's experiments, who fays, "the air over the river Ouze was conftantly purer than that of the garden by two or three degrees." Philofophical Tranfactions, vol. 68. And in the fame paper he obferves, that the fame happened with the air of the fofs when the marfhes were overflowed.

When I firft engaged in thefe experiments, it was my intention to perform them only on fea air; but I foon found it neceffary to repeat them on land air, for the fake of comparifon. The fubject increafed on my hands: The atmofphere of marfhes prefented itfelf as worthy of ferious inveftigation; I therefore performed fome experiments upon it: but proper length of time is neceffary to their repetition, and for this reafon I muft omit them for the prefent, and merely relate thofe I performed on the air of this city, its environs, and on the ocean.

I fhall firft proceed to the enquiry, whether the atmofphere differs in purity in different fituations on land?

The opinion that the air is purer in the country and on the tops of mountains than it is in towns, is adopted by many; therefore, in afferting the contrary, we muft prepare to meet with oppofition, particularly from thofe who have formed opinions from reafoning alone, unfupported by experiments. In doubtful matters, it is chiefly by the clafhing of opinion that truth is finally difcovered. This fhall be both my confolation and apology, if the refult of my experiments fhall be found to have induced me to differ from others. Neverthelefs, it will afford me confiderable fatisfaction to agree with thofe whofe decifions reft upon the fame firm bafis; I fhall, therefore, briefly mention the authors who agree with me in opinion.

Dr. Prieftley concludes from his own experiments, that the difference of the air in different places, fuch as is indicated by a mixture of nitrous air, is in general very inconfiderable. He mentions, that the air of Harthill, near Manchefter, and that of Wiltfhire, were about the fame.

The compilers of the Encyclopædia fay, "that the general mafs remains upon all occafions pretty much the fame." And Scheele is much of the fame opinion.

But the accurate Fontana fpeaks with more confidence, and is more explicit. His affertions are founded upon the refult of many experiments, and he is inclined to believe, that the flight variations mentioned by fome philofophers, are rather to be attributed "to the fallacious effects of uncertain methods," than to any real difference in the air itfelf. He found the air of Iflington and London to fuffer an equal diminution from the mixture with nitrous air. Air taken at different heights in London

London and Paris did not differ in purity. Air at the height of 313 and 202 feet in London, differed ſcarcely at all; and no difference was perceptible between the air of theſe heights and that of the ſtreet adjoining.

The more I reflect on this ſubject, the more I am inclined to adopt the following ſentiment of this laſt mentioned gentleman, viz. "The difference in the purity of the air at different times, is much greater than the difference between the air of the different places." Indeed, moſt of the experiments related by Dr. Ingenhouſz alſo tend to confirm it. In general, the difference in the air of different places at the ſame time was by no means conſiderable.

I ſhall now with more confidence relate the experiments I myſelf performed; but previous to this recital I ſhall give a brief account of the method I purſued.

It is neceſſary to remark, that every experiment I ſhall relate is the reſult of, at leaſt, two different trials.

Moſt authors who have engaged on this ſubject uſed eudiometers of a different conſtruction; I adopted the moſt ſimple as the beſt. Thoſe who deſire a particular deſcription of theſe inſtruments, may be ſatisfied by referring to the Encyclopædia, and different parts of Dr. Prieſtley's Treatiſe on Air. Mine is as follows:*

I had a glaſs tube about fourteen inches in length, and in diameter nearly half an inch, provided with a graduated ſcale, made ſo as to ſlide upon the tube, up or down, as occaſion required. This ſcale was divided into one hundred equal parts.

My meaſure was a ſmall ſmelling bottle, containing ʒj. and gr. xvj. of clear pump water. The ſpace occupied in the tube by a bulk of air which this meaſure could contain, was equal to the hundred diviſions of the graduated ſcale.

My water trough on board of the ſhip was the common water bucket; on ſhore, it was a common houſe bucket or tub.

The

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* The atmoſphere is proved by inconteſtable experiments to conſiſt in general of,

Oxygen gas,	0,27.
Azotic gas,	0,72; and
Carbonic Acid,	0,01.

It is a fact well known to chemiſts, that nitrous air will combine with oxygen gas, and form a compound, viz. the nitric acid. As theſe two gaſes combine, they aſſume a ſtate approaching nearer to that of a ſolid, and conſequently occupy leſs ſpace than they did before their union. Upon this diminution of bulk depends our eſtimation of the purity of the air: The greater the contraction, the purer we ſuppoſe the air under trial.

The nitrous gas was prepared from diluted nitric acid and brafs filings.

At fea, I ufed fea water in the trough; on land, common pump water; for from different trials made by Dr. Ingenhousz, it is evident this circumftance could not produce a variation in the refult of the experiments.

My method of operating is as follows: After having introduced two meafures of the air, whofe purity I defired to afcertain, into the glafs tube, I introduce one meafure of nitrous gas; then, fuffering the tube to remain undifturbed for about a minute, I noted down how far the water afcended without agitation; this is what I have called, upon mixture: I then agitated the tube three fucceffive times, after the manner of M. de Sauffure, and noted how high the water rofe. In many inftances I added a fecond meafure of nitrous gas, and thereby completely faturated the air under examination.

I was particularly cautious of avoiding miftakes from hurry or inattention, and took fome pains to guard againft all the circumftances Dr. Ingenhoufz mentions as liable to produce a variation in the refult of experiments of this kind.

My firft experiment on land air was performed Auguft 2, 1796. Two meafures of air in the yard of my lodging, when mixed with one meafure of nitrous air, left upon mixture 2.48 of a meafure; and after fhaking the tube 1.79. I then added another meafure of nitrous air and 2.65 remained.

I then fubmitted air to the teft of the eudiometer, which I had previoufly collected in different ftreets of this city, viz. in Water between Market and Arch Streets; in Spruce near Fourth Street; in Chefnut near Fifth; and, in Market between Second and Third Streets. Each of thefe airs gave nearly the fame refult, and generally agreed with that of the air of my lodging: None of the experiments fhew a difference of 0.02 of a meafure.

Similar experiments I have fince repeated and the refult was the fame.

Auguft 3d. I collected air on the top of the hill whereupon Dr. Smith's Obfervatory ftands, at the Falls of Schuylkill, five miles from Philadelphia. In another vial I received air from above the middle of the road directly at the foot of the hill. And immediately on my return home I fubmitted them and the air of the yard to experiment, and found them to agree exactly as follows

Upon mixture 2.48.

After fhaking the tube 1.78 and upon adding a fecond meafure of nitrous air, 2.63 remained.

Auguft

August 5th. I collected air from above two different marshy situations immediately below the rope-walks to the south of this city. It is of consequence to remark that these marshes are overflowed by the tide. Another vial I filled immediately before entering the city in Front Street. These airs suffered an equal diminution from a mixture of nitrous gas, viz. 2.47 upon mixture; after shaking the tube 1.79; and after adding a second measure of nitrous gas 2.64 remained.

The air near my lodging yielded upon mixture 2.49; after shaking the tube 1.78; and upon the addition of a second measure of nitrous gas 2.62.

I performed some experiments on air collected in other situations about the city; but finding the result so much the same as those above related, I did not make any note of them, and remain perfectly satisfied that M. Fontana's assertion is well founded.

To these experiments I will subjoin those I made on the ocean during a passage from Bourdeaux to Philadelphia. It appeared to me preferable to connect them in the form of a table, as thereby I should avoid a needless repetition; and place before the reader a short though accurate view of all the experiments at the same time.

The experiments I performed on the river Elk and bay of Chesapeak perfectly agree with each other; and the result was the same as those performed on the 7th of July, &c. The wind blew from the north, and the sky was partially cloudy. They were performed in August last.

My experiments at sea sufficiently prove that the atmosphere is considerably purer there than it is on land. Though there are some trifling differences in the results of several experiments, I have no reason to believe that they were owing to the different situation in point of latitude or longitude in which they were performed. I can form no system respecting such variations. Winds, temperature, rain, &c. do not seem to have produced them. As they did not observe any regularity in their occurrence, they may perhaps be attributed to certain unperceived errors which are unavoidably attendant on such trials.

[To be continued.]

On

On the Progress of Galvanism.

OUR Readers have already had their attention turned to the curious and interefting fubject of Galvanic Electricity.* Since the period of thofe communications, the fubject has been profecuted with great induftry and ability at the Royal Inftitution; and we have reafon to believe, that all the improvements or difcoveries of importance, fince that time, claimed by our neighbours, have been made in this country.

" When the great intenfity of the fhock is confidered, and at the fame time the minutenefs of the fpark, and weak appearances of other electrical phenomena, the neceffity of the hands being wetted to receive the fhock, its not breaking, though fo very ftrong, through any confiderable fpace of air, nor occafioning any or fcarcely any report, as well as from the different kind of fenfation it produces, it is not furprizing that feveral philofophers fhould doubt its being an electric phenomenon, or if it be, that it is much modified by unknown circumftances. We ought to confider, however, that it is the character of electric fhocks, which are felt to equal diftances from the extremities of an animal, that thofe which are produced by a fmall quantity of electricity at a high intenfity, caufe a more fudden and tranfient fenfation than thofe produced by a much larger quantity at a low intenfity, and therefore probably moving more flowly. When a quantity of electricity, for inftance that produced by fix turns of a machine, is put into a fmall Leyden phial, a fmart quick fhock will be felt, accompanied with a loud report, and the electric fluid will move through a confiderable fpace of air, and fhew a very lucid fpark. The electrometer will likewife indicate a high intenfity before the difcharge. But if the fame quantity be put into a phial having four times the coated furface, the electrometer will only fhew $\frac{1}{4}$ of the intenfity: the fpark will break through only $\frac{1}{16}$ part of the fpace that it did in the former inftance, accompanied with a very feeble fpark; but the fhock will be the fame to a perfon who receives it through his body, becaufe the fame quantity of electric fluid muft pafs through him, though it is eafy to conceive that the fenfation will be different, as it probably moves with lefs velocity in the laft cafe than in the firft. If, inftead of a phial, we fuppofe the electricity to be difperfed over the furface of a large

* Journal, Vol. I. p. 53, and Vol. IV. p. 115.

battery, the intenſity diſcoverable by the electrometer will be very ſmall indeed: the ſtriking diſtance will be almoſt nothing, and the ſpark much diminiſhed, though the ſhock muſt ſtill be equally powerful. It would appear from ſome ingenious calculations made by Mr. Nicholſon, that the celerity with which the Galvanic pile produces electricity, is two hundred times more rapid than what can be produced by the labour of one man with a powerful electrical machine, while at the ſame time its intenſity is ſo low, that the ſhock may be conſidered as produced much more by the maſs, or quantity of electricity, than by the velocity of its motion.

" Some of the moſt curious properties of this apparatus of Signor Volta ſtill remain to be mentioned: we mean its action as a chemical agent, in which its effects are much more powerful than common electric machines, and uncommonly intereſting. When the Galvanic influence is directed in a proper manner through water, that fluid is decompoſed, and ſeparated into its component parts, *viz.* oxygen and hydrogen, which appear in the form of gas, and in their proper proportions. This curious fact was firſt noticed by Mr. Carliſle and Mr. Nicholſon. The circumſtances which led to this diſcovery are deſcribed by the latter gentleman in his Journal, Vol. IV. p. 182, which may be conſulted by the reader. We ſhall content ourſelves here with pointing out the method of making the experiment. If a glaſs tube, about ſix inches long, and half an inch in diameter, be filled with diſtilled water, which is confined in it by means of two corks, and if two copper or iron wires, communicating with the top and bottom of the pile, be made to paſs through the corks, ſo as to be about two or three inches diſtant from each other; if the loweſt piece of the pile be ſilver, a fine ſtream of gas, in very minute bubbles, will immediately iſſue from the loweſt wire, and riſe to the top, where they will collect: this wire will remain very clean and bright. From the upper wire, which is connected with the zinc end of the pile, no gas is obſerved to riſe; but the wire itſelf oxidates very quickly, and the oxid gradually ſubſides in the form of very fine clouds: during this operation the water is diminiſhed in quantity. If this gas be examined in the uſual manner, it is found to be very nearly pure hydrogen. In this experiment it would appear that the hydrogen is ſeparated from the water, and converted into a gaſeous ſtate, by the wire connected with the ſilver end of the pile, while the oxygen unites itſelf with the wire connected with the zinc end. When the pile is inverted, the hydrogen gas ariſes from the upper wire, and the lower oxidates. That this effect is produced by the Galvanic influence is evident; for on making a communication, by means of a conductor, between the

upper

upper and lower extremities of the pile, the emiſſion of gas is ſuſpended, but proceeds again, as ſoon as the communication is removed.

"If two unoxidable wires, as for inſtance wires of gold or platina, be uſed, then a ſtream of gas iſſues from each, and the water is diminiſhed. This gas is found to be a mixture of hydrogen and oxygen, and explodes violently on the approach of an ignited body.

"In order to procure theſe two gaſes ſeparate from each other, to eſtimate their quantities, and examine their nature more particularly, Mr. Cruikſhank took a glaſs tube, ten inches in length, and by means of a blow-pipe bent it in the middle until the legs formed an acute angle, reſembling the letter V. While the glaſs was red-hot, he contrived to blow an opening at the angle, about one-tenth of an inch, or a little more, in diameter. Two gold wires, paſſed through corks ſecured by cement, were introduced into the legs, and brought within an inch of each other at the bend: the tube was then filled with diſtilled water, and a finger placed on the opening at the angle, to prevent the fluid from eſcaping. It was placed in a tea-cup containing water, with the angle downwards, the legs having an inclination of about forty-five degrees. The extremities of the wires being then brought into contact with thoſe of the pile, a quantity of gas was diſengaged from both; but by far the moſt from that connected with the ſilver. By this contrivance, the gaſes from the two wires were obtained perfectly diſtinct, each gas aſcending in the leg of the tube which contained its generating wire. On examining the gaſes, that from the wire connected with the ſilver end of the pile was almoſt pure hydrogen, and that from the zinc end almoſt pure oxygen, in the proportion of two parts by meaſure of the former to one of the latter, or nearly in the proportion in which they exiſt in water.

"When the glaſs tube is filled with diſtilled water, to which a little tincture of litmus is added, and a communication made by gold wires, a quantity of gas ariſes from both wires; but in greateſt quantity from that connected with the ſilver. In a few minutes a fine red line is ſeen, extending upwards from the extremity of the zinc wire*, and in a ſhort time the whole fluid near the point of this wire becomes red: the fluid, however, about the ſilver wire looked of a deeper blue than before, the ſlight tinge of purple being deſtroyed.

* For the ſake of brevity, the wire connected with the zinc end of the pile is called the zinc wire, and that connected with the ſilver end, the ſilver wire, as was done by Mr. Cruikſhank.

"If the tube be filled with diftilled water, tinged with tincture of Brazil-wood, the fluid furrounding the filver wire, particularly towards its extremity, becomes purple; and this tinge increafes fo faft, that the whole fluid furrounding this wire foon affumes as deep a colour as can be produced from ammonia, while the portion of the fluid in contact with the zinc wire becomes very pale, and almoft colourlefs.

"Thefe experiments would feem to fhew that an acid, probably the nitrous, is produced at the wire proceeding from the zinc, which may arife from the union of the oxygen of the decompofed water with the azote of the common air diffolved in the water: at the fame time, an alkali, probably ammonia, appears to be formed at the filver wire, moft likely from the union of the nafcent hydrogen of the decompofed water with the azote of the atmofpheric air.

"In reflecting on thefe experiments, it appears, that in fome of them the water is decompofed; but how this is effected is by no means eafy to be explained. For example, it feems extremely myfterious how the oxygen fhould pafs filently from the extremity of the filver wire to that of the zinc wire, and there make its appearance in the form of gas. It is to be obferved, likewife, that this effect takes place whichever way the wires are placed. The theory offered to the public by Mr. Cruikfhank is certainly ingenious. He thinks that the fimpleft and eafieft mode of explanation would be to fuppofe, that the Galvanic influence (whatever it may be) is capable of exifting in two ftates, that is, in an oxygenated and de-oxygenated ftate. That when it paffes from metals to fluids containing oxygen, it feizes their oxygen, and becomes oxygenated; but, when it paffes from the fluid to the metal again, it affumes its former ftate, and becomes de-oxygenated. Now, when water is the fluid interpofed, and the influence enters it from the filver fide, de-oxygenated, (and he fuppofes that it always paffes from the de-oxygenated to the oxygenated fide), it feizes the oxygen of the water, and difengages the hydrogen, which accordingly appears in the form of gas; but, when the influence enters the zinc wire, it parts with the oxygen with which it had formerly united, and this either efcapes in the form of gas, or unites with the metal to form an oxyd, or combined with fome of the azote contained in the water may form nitrous acid.

"It is a well known chemical fact, that hydrogen gas, in its nafcent ftate, reduces the oxyds of metals; it was natural to expect, therefore, that by filling the glafs tube with a metallic folution, the hydrogen would feparate the oxygen from the metallic oxyd, and procure the metal in its pure ftate. Accordingly, when the tube is filled with a folution of acetite of lead in

in diſtilled water, and a communication made in the uſual way, no gas is perceived to iſſue from the ſilver wire, but in a few minutes beautiful metallic needles are perceived on the extremity of this wire: theſe ſoon increaſe, and aſſume the form of a fern, or other vegetable. The lead thus ſeparated is perfectly in its metallic ſtate, very brilliant.

"When a ſolution of ſulphat of copper is employed, the copper is precipitated in its metallic ſtate, but inſtead of appearing in cryſtals, it forms a kind of button, which adheres firmly to the end of the wire.

"On making the experiment with a ſolution of nitrat of ſilver, the ſilver is precipitated in the form of a beautiful metallic bruſh, the metal ſhooting into fine needle-like cryſtals.

"Theſe phenomena may be explained on Mr. Cruickſhank's hypotheſis, by ſuppoſing the Galvanic influence, in paſſing from the ſilver wire, ſeizes the oxygen of the metallic oxyde, and afterwards depoſits it on entering the zinc wire. In this caſe, no gas ſhould appear at the ſilver wire; but, when a perfect metal is employed, oxygen ſhould be diſengaged from the zinc one; and this is exactly what takes place. What Mr. Cruickſhank conſiders, however, as the ſtrongeſt argument in favour of this hypotheſis, is, that all fluids which do not contain oxygen, are incapable of tranſmitting the Galvanic fluid; ſuch as alcohol, ether, fat, and the eſſential oils, as he has proved by direct experiments: but, on the contrary, that all thoſe which do contain oxygen, conduct it more or leſs readily, as all aqueous fluids, metallic ſolutions, and acids, more eſpecially the concentrated ſulphuric acid, which it decompoſes. By this theory, alſo, we can eaſily explain the oxydation of the zinc plates in the machine, where the Galvanic fluid, in paſſing through the different plates, appears to be alternately oxygenated and de-oxygenated.

"Having found that metallic ſolutions were decompoſed by the Galvanic influence, Mr. Cruickſhank wiſhed to aſcertain if their ſolution in alkalies, and more particularly ammonia, could be decompoſed in the ſame way. For this purpoſe, to a dilute ſolution of the nitrat of ſilver he added ſome pure ammonia, until the mixture ſmelled ſtrongly of the latter ſubſtance. The mixture was introduced into the glaſs tube, and ſilver wires applied. When the tube was placed in the circle of communication, a very rapid production of gas took place from the wire connected with the ſilver extremity of the pile, although little or none eſcaped from the zinc wire during the proceſs.

"After ſome time a quantity of greyiſh flakes, evidently metallic ſilver, were ſeparated by the wire which gave out the gas, and a dark grey powder was ſeparated from the zinc wire.

After some hours, the apparatus was removed: at this time a considerable quantity of metallic silver was deposited, and the zinc wire was encrusted by a blueish-black substance. On endeavouring to remove this crust with the finger, part of it exploded, though still moist: the wire was found to be very much corroded and full of holes. Next morning, the powder which adhered to the wire, being dry, was touched with a knife, when it exploded with a very considerable noise. There can be little doubt, that these explosions proceeded from the fulminating silver of Berthollet, which must have been formed at the zinc wire. The experiment was repeated with pure ammonia, instead of the solution of silver, which answered almost equally well; for the silver of the zinc wire, after being corroded, was taken up by the ammonia, and afterwards deposited, in its metallic form, by the silver wire. A small quantity of fulminating silver was likewise formed.

"A quantity of pure ammonia being put into the tube, and platina wires being introduced as conductors, a rapid production of gas took place from both wires, which, on being examined, was found to consist of hydrogen and azotic gas, very nearly in the same proportion as they exist, according to Berthollet, in ammonia. Here, undoubtedly, the ammonia was decomposed by the Galvanic influence. The oxygen disengaged at the zinc wire uniting with the hydrogen, while the azote of the ammonia escaped in the form of gas, and became mixed with the hydrogen, disengaged at the same time from the silver wire.

"From these experiments it will be perceived, that the Galvanic influence is a very powerful chemical agent, and that, by means of it, decompositions may be made much more quickly, and with infinitely less trouble, than by an electrical machine. It might, indeed, probably be employed with success in the analysis of minerals, more particularly in separating lead, copper, and silver, from their different solutions.

"Since the above account was drawn up, some very interesting facts have been published by Mr. Davy, superintendant of the Pneumatic Institution at Bristol, of which, as they throw considerable light on the phenomena of Galvanism, we shall give an abstract.

"From the experiments made by Mr. Davy, he has been led to the following conclusions:

"1. That the zinc in the plates of a Galvanic pile undergoes no oxydation at common temperatures, so long as the water in contact with it is *pure*, or holds in solution no oxygen gas, no nitrous gas, and no acids. A pile, in which the cloths were moistened with distilled water which had been boiled, being introduced

troduced into a veſſel of water, and the acceſs of atmoſpheric air cut off, on being examined after two days, the pieces of zinc were found unoxydated: the zinc, though in contact with pure water, was not oxydated, when the pile was placed in hydrogen gas, or *in vacuo*.

" 2. The oxydation of the zinc plates of the Galvanic pile takes place whenever the water in contact with them holds atmoſpheric air, oxygen, nitrous gas, nitrous acid, muriatic acid, &c. in ſolution. Mr. Davy found, likewiſe, that the oxydation took place much more rapidly in pure oxygen gas than in the atmoſphere.

" 3. When the zinc in contact with water, holding in ſolution ſubſtances containing looſe oxygen, or acids, becomes oxydated, theſe ſubſtances become changed, and exert ſome chemical affinities.

" 4. The Galvanic pile ſeems incapable of acting when the water between the pairs of plates is pure, or contains no oxygen, or ſubſtances containing oxygen.

" 5. The pile acts when the water between the plates holds in ſolution atmoſpheric air, oxygen, nitrous gas, nitrous acid, or muriatic acid.

" 6. The power of action of the pile appears to be proportional to the power poſſeſſed by the conducting fluid ſubſtance between the plates to oxydate the zinc. This appears from the zinc oxydating leſs rapidly in nitrous gas than in atmoſpheric air, and leſs rapidly in atmoſpheric air than in oxygen.

" From theſe facts it appears, the pile of Volta acts only when the conducting ſubſtance between the plates is capable of oxydating the zinc, and that, in proportion as a greater quantity of oxygen enters into combination with the zinc in a given time, ſo in proportion is the power of the pile to decompoſe water, and to give the ſhock greater; and it ſeems not unreaſonable to conclude, that the oxydation of the zinc in the pile, and the chemical changes connected with it, are ſome how the cauſe of the electrical effects produced by it.

" Aſſuming the truth of this concluſion, it was eaſy to conceive, that a pile, much more powerful than any hitherto conſtructed, might be made, particularly on the ſuppoſition that the decompoſition of the interpoſed water was not eſſential to the proceſs. Plates of zinc and ſilver, 1,2 inches ſquare, were faſtened in pairs by reſinous cement: eighteen of theſe pairs were connected with each other by cement, and ſo incloſed by it, as to leave water-tight partitions open at one ſide only, between each pair of plates. When muriatic acid was poured between the partitions of this machine, the plates being perpendicular to the horizon, it acted very powerfully: its capability

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of decompoſing water, and giving the ſhock, being at leaſt equal to that of a common pile of ſeventy plates. Diluted nitrous acid made it act ſtill more powerfully. When the partitions were filled with water, its action was barely perceptible. Concentrated nitrous acid was poured into them: the ſhock was ſo powerful as to benumb Mr. Davy's fingers for ſome ſeconds, and he did not dare to take another. In a ſecond experiment with ſtrong nitrous acid, he uſed only five pairs of plates, and the ſhock was full as powerful as from a common pile of thirty plates. Three pairs of plates, with nitrous acid, gave a very ſenſible ſhock.

"We have now given as diſtinct an account as was conſiſtent with the room alloted for this article, of the phenomena of Galvaniſm. The diſcovery of Volta, by which its force may be ſo greatly augmented, may be reckoned among the moſt important with which philoſophy has lately been enriched; and there can be little doubt, from the ardour with which experiments are now making, that great light will be thrown by it both on electricity and chemiſtry. May we not likewiſe venture to predict, that the preſent machines, though very ſtrong, will, in proceſs of time, give way to others, both vaſtly more powerful and convenient? Many perſons remember the time when the beſt electrical machine wa a tube of glaſs, which was rubbed by the hand with a piece of leather. The celebrated Franklin uſed a machine of this kind, with which he made experiments before his ſelect friends. How defective was this, both in power and convenience, when compared with the common machines in uſe; and is it by any means improbable, that the pile of Volta, which is now viewed with ſo much aſtoniſhment, may in a ſhort time give way to improvements equally great? The energy of Galvaniſm, as a medical agent, remains ſtill to be inveſtigated; but when we conſider t e effects which a very ſmall quantity of this influence produces on the muſcles of an animal, even after it is deprived of life, it is not, we think, unreaſonable to predict, that the powerful current ſent for a conſiderable time through a diſeaſed part, may be productive of the beſt effects.

"Anatomiſts have diſcovered, that the electric organs of the torpedo and electrical eel conſiſt of numerous membranous columns, filled with a great number of plates, or pellicles, in the form of thin diſcs, ſeparated from each other by very ſmall intervals, which are filled with a kind of moiſture or fluid. This organ being formed of conducting ſubſtances, can neither be referred to the electrophorus, the condenſer, or the Leyden phial, nor to any other ſpecies of electrical machine, excitable by friction, or any of the means by which inſulated bodies are electrified. The organs of the fiſh very much reſemble the pile of Volta

Volta in their conſtruction and their effects: it is compoſed of conductors, is capable of acting in water, and will give inceſſant or re-iterated ſhocks like them; in ſhort, it may be looked upon as an artificial torpedo.

"The electrical eel and torpedo will give a ſhock to a perſon dipping his hands in the water in which they ſwim, though this ſhock is much more feeble than thoſe experienced when theſe fiſhes are graſped in the hands out of water. In the ſame manner, if a wire coming from the bottom of the pile be inſerted into a baſon of water, and another from the top terminate in the ſame baſon, at ſome diſtance from the firſt, on immerſing the hand in the water a ſhock will be perceived, though not ſo ſtrong as when the water does not intervene. This can only be explained on the ſuppoſition that the living body is a better conductor of this influence than water; the hand therefore offers an eaſier and freer paſſage to it than water; part of it is conveyed through it, though part will ſtill paſs through the water, which will diminiſh the ſtrength of the ſhock.

"It is probable, that when the torpedo wiſhes to give a ſhock to an animal which is in contact with it, or which has any part immerſed in the water in which it ſwims, it need only bring into contact ſome parts of the electric organ, which may be ſeparated by ſome ſlight interval. In the ſame way, if the ſmalleſt interruption takes place in the parts of either of the wires, in the experiment above deſcribed, no ſhock will be felt on putting the hand into the baſon; but the inſtant the contact is made complete, the ſenſation will be experienced.

"A very excellent account of Galvaniſm, previous to the diſcovery of Volta's pile, may be found in the Supplement to the Encylopædia Britannica; alſo in Fowler's Eſſay on Animal Electricity.—Valli's Experiments on Animal Electricity.—A paper by Dr. Wells, in the Philoſophical Tranſactions for 1795.—A Diſſertation by Dr. Chriſtopher Heinrick Pfaff, *de Electricitate Animali*, publiſhed at Stutgardt, 1793.—Fabroni on the Chemical action of metals upon each other at the common temperature of the atmoſphere, and upon the explanation of certain Galvanic phenomena, Journal de Phyſique, Tom. VI.

"For an account of Volta's paper, ſee Philoſophical Transactions, 1800, part 2d.; alſo Nicholſon's Journal, vol. vi. p. 179, 187, 223, 241, 254, 275, 313, 326, 337, 388, 394, 472."

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

ON perusing your useful Journal for last month, I observed two cases of Jaundice very accurately described by Dr. Crichton, proceeding from a cause, as the Doctor supposed, hitherto undescribed.

It is very improbable that such an insignificant publication as my Thesis, (De Ictero,) published in the year 1799, should ever fall into the Doctor's hands, I therefore take the liberty of mentioning that I there gave, as one of the remote causes of jaundice, a schirrus of the pancreas, and had the satisfaction of having my opinion *fully* corroborated by a preparation of that organ, in a schirrous state, being shewn by Dr. Monro of Edinburgh, at his lecture, and which had been the cause of a jaundice of two years standing, as appeared upon dissection.

I am sure I need not make any apology to Dr. Crichton for the above statement, being well convinced that he will receive satisfaction from any further testimony in favor of a medical fact he has so correctly given to the world. I am, &c.

North Allerton, Aug. 6, 1801. W. B. BAYLEY.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

IN the last number of the Medical and Physical Journal, you say in a note, p. 121, that "when the Vaccine virus is taken after the areola is fully formed, it is not to be depended upon as a preventive of the Small-pox, although it will produce a disease imitative of the true Vaccine."

Perhaps this sentence may not appear perfectly intelligible to other practitioners any more than it does to me. I would therefore wish to enquire, whether the virus taken after the full formation of the areola *never* produces the genuine Cow-pox, or whether it only *sometimes* fails; and, in this case, by what characteristics the imitative disease is to be distinguished from the genuine?

I am anxious to procure information on this subject, as I have lately, from circumstances, been induced to inoculate a patient with virus taken on the eleventh day after the incision, and after

after the full formation of the areola. This inoculation produced a diſeaſe in every reſpect ſo ſimilar to the genuine vaccine, that I was perfectly ſatisfied of my patient's ſecurity from variolous infection, till the above note raiſed ſome doubts in my mind, which I hope you will have the goodneſs to diſpel.

I can recollect no variance whatever from the genuine diſeaſe, of which I have ſeen a very ſufficient number of caſes, unleſs the ſeparation of the ſcab ſooner than uſual is to be reckoned one. The patient, a child about eight weeks old, was inoculated on the 1ſt of the month, and the ſcab came off with the ſlighteſt poſſible force on the 17th. In every other reſpect the appearance was ſtrongly characteriſtic of the Cow-pox, the infection, inflammation, and every ſymptom took place in the moſt regular and ſatisfactory manner.

Your attention to this note in your next number, will confer an obligation on, Gentlemen, your conſtant reader.

M.

Our deſign in the Note above alluded to, was to inculcate the advice ſo often given by Dr. Jenner, to uſe none but early virus; and alſo to inform our readers that matter taken after the areola was fully formed had produced a diſeaſe, not at preſent to be diſtinguiſhed from the true Vacciola by any deſcription, but which failed to prevent the ſmall-pox. We do not believe that it will always fail, but we think it ought never to be truſted. Ingenious artiſts are now at work, in the hope of being able to give accurate repreſentations of the true and ſpurious puſtules.

In the mean time, why ſhould practitioners expoſe themſelves and patients to hazard, when early virus can be ſo eaſily procured? Edit.

To the Editors of the Medical and Phyſical Journal.

Gentlemen,

In the relation of a caſe of Vaccine Inoculation by Mr. Pole, in page 340 of the Journal for April, he remarks, that in conſequence of no progreſs being obſervable in the inflammation of the punctured part on the eighth day, he inoculated with the Variolous matter, and that in three days after, each inciſion aſſumed the appearance of having taken effect. Both went on together until the *areola*, or inflammatory circle of the Vaccine ſurrounded that of the Variolous puncture, and thus influencing the conſtitution, arreſted the further progreſs of Variolous infection on the fifth day. (An important medical fact.) The external marks of the latter now died away, the Vaccine only affecting the patient, and running its ordinary courſe in all

all refpects, with its flight but ufual fymptomatic operation on the fyftem.

Mr. Pole then obferving of the fact of the inflammatory progrefs of the Vaccine Inoculation remaining almoft imperceptible or inactive for the firft eight days, that he did not remember an inftance to have occurred before, I extract for his information, as well as for medical practitioners in general, the following note from a memorandum-book I keep of patients inoculated with the Vaccine matter; viz. " November the 10th, 1800. Inoculated Mr. Barland's child, of Princes Street, Lothbury, ten months old; had no appearance of the Vaccine Inoculation having taken effect until between the eighth and tenth days; inflammation of the punctured part then came on, and ran the ufual courfe." The arm afterwards, I remember, exhibited a remarkable fine puftule: but when I called on the eighth day, fo little and hopelefs was the appearance of the infection having taken place, that I told the parents I fhould inoculate the child again on the day but one after, viz. on the tenth day. I paid my vifit for that purpofe, and was very agreeably furprifed to find the punctured part inflaming, and proceeding as above related.

I beg leave to take this opportunity to notice the like phænomenon in the Variolous inoculation, which others no doubt may have obferved. Both my partner, Mr. Price, and myfelf, have met with feveral inftances in the courfe of twenty years practice, of the Variolous matter remaining inactive on the incifed part for eight or ten days or longer, without fhewing any appearance or advancement of inflammation, more than might be excited by irritation on the fkin with any cutting inftrument. After fuch period the inflammatory rednefs and puftular prominence have began to take place, and gone on regularly in their natural courfe, the febrile and eruptive fymptoms of the Variola fucceeding in the uniform and ufual way.

New Basinghall Street,
June 23, 1801. THEOPHILUS DYSON, Surgeon,

Observations on Phlegmatia Dolens, *by* Dr. Hull.

[Continued from our laft, pp. 135—151.]

MR. WHITE, in the former part of his *Inquiry*, *&c.* has never alluded to a divifion of the lymphatics, which defcend into the cavity of the pelvis, in confequence of a fharp line or ridge of the os pubis; but thinks it probable that " the *predisponent*

ponent cause may be a weakneſs of the coats of the lymphatics in ſuch ſubjects only as have theſe veſſels formed into one principal trunk under Poupart's ligament," p. 55; and he intimates, that the remote cauſe of the diſeaſe under conſideration, is a rupture of one or more lymphatic veſſels, "owing to the child's head preſſing the lymphatic veſſel or veſſels, which ariſe from one of the lower extremities againſt the brim of the pelvis during a labour pain, ſo as to ſtop the progreſs of the lymph," p. 49.

In my Eſſay on Phlegmatia *dolens*, I have offered ſome objections to the above account of the production of the diſeaſe, and to other parts of the Theory, as given by Mr. White in his former publication. Theſe objections he has endeavoured to obviate, in the Second Part of his *Inquiry, &c.* p. 78, &c. and we ſhall now ſee with what ſucceſs.

My *firſt* objection is, that "*there is no evidence of the burſting of a lymphatic during labour, much leſs of its healing in ſuch a manner as to render the canal impervious, or occaſion a diminution of its diameter.*" Mr. White replies, "There is the evidence of Mr. Aſtley Cooper, that the contractile power of the abſorbents is ſufficient to occaſion a rupture of their coats by tying them." Now, Mr. Cooper's Experiments, which are referred to by Mr. White in proof of this aſſertion, only ſhew this, viz. that the *receptaculum chyli* of a dog, which is large and thin in this animal, is generally, but not always, found ruptured when the thoracic duct is tied. They do not ſhew that *the lymphatic of the extremities* have been ruptured, in conſequence of a ligature paſſed round them, or the thoracic duct; nor has Mr. White adduced a ſingle experiment to prove that this has ever been the caſe. There is no part of the lymphatic ſyſtem of the lower extremities of a woman, that bears the leaſt analogy in its ſtructure to the *receptaculum chyli* of a dog; conſequently, what Mr. White has here adduced, is totally irrelevant. Mr. A. Cooper obſerves, that at the lower part of the neck, the thoracic duct is generally divided into two or three trunks, which again become united into one; and he thinks it probable, that this may ſerve as a proviſion againſt diſeaſe, from what happened to him in tying the thoracic duct of a dog; for having included only one of thoſe diviſions, the two others carried the chyle into the blood, and the animal recovered.* I have lately been informed by Mr. A. Cooper, that the legs of the dog, which was the ſubject of the above experiment, did not become ſwelled.

The

* Medical Records and Reſearches, p. 101 and 102.

The fame ingenious anatomift tied the extremity of the thoracic duct near the left jugular and fubclavian veins in another dog: The duct became much diftended, and the dog died forty-eight hours after. On diffection, the receptaculum chyli was found ruptured, and an effufion of chyle had taken place. The abforbent veffels of the hinder extremities and organs of generation were diftended, but not in an equal degree with thofe of the left fore leg and left fide of the neck; one of the latter was larger than a crow's quill.* None of the legs of this dog were materially fwelled, as he fince informed me.

My fecond objection is, that "*there is not the leaft reafon to fuppofe, that a lymphatic trunk ever burfts in confequence of the tranfient obftruction of the lymph, induced by a labour pain. If this temporary obftruction, which does not occafion any fenfible enlargement of the limb, could produce the rupture of a lymphatic, we might expect, from the long continuance of the obftruction, which takes place agreeably to Mr. White's hypothefis, that every lymphatic in the limb would burft, before the limb had arrived at half the fize which we know it attains in the difeafe under confideration.*" Mr. White replies in the following words: "Mr. A. Cooper has likewife proved, (See note, p. 64) that the tranfient obftruction of the lymph, for a few minutes, will burft an abforbent. Whenever the fwelling of the limb lofes its elafticity, and pits upon preffure, it is probable that many of the lymphatic veffels are burft." This reply does not obviate, or even bear upon my objection. Mr. A. Cooper's experiment, to which Mr. White alludes, only proves, that by the compreffion of the thoracic duct for a few minutes, the *receptaculum chyli* of a dog may be found ruptured.†

My *third* objection is, "*If the fwelling of the limb be occafioned by a congeftion and coagulation of lymph in the abforbents, how is it poffible it could ever fubfide? How is it poffible, that the fwelling fhould be fo nearly even as it is found to be at firft? And why can we not trace the gradual increafe of the lymphatics in every inftance, from a flightly enlarged or varicofe ftate, to that extreme degree of diftention at which they muft arrive, in order to contain all the fluid neceffary for the production of this enormous fwelling of the limb.*" Mr. White, in anfwer to the firft query, fays, "The abforbents are certainly as capable of abforbing coagulated lymph as coagulated blood, but they are equal to ten times more than that." I am ready to grant, that the abforbents, *when their canals are pervious*, are capable

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* Medical Records and Refearches, p. 107.
† Ibid, p. 110.

ble of abſorbing not only coagulated blood, but fleſh and bone; nevertheleſs, when theſe are completely gorged with coagulated lymph, as Mr. White ſuppoſes them to be, I do not ſee how they are to be freed from it. To the ſecond query he replies, "The ſwelling being ſo nearly even, is owing to the ſuperficial lymphatic veſſels in the extremities of the human body being ſo very numerous, fourteen trunks frequently accompanying one vein:" But from the obſervations of Soemmering, and other acute anatomiſts, we find, that the lymphatics, when much diſtended, do really become varicoſe and perceptible in limbs not affected with Phlegmatia *dolens*. Why then cannot they be conſtantly perceived in a varicoſe ſtate in limbs affected with this diſeaſe? If the lymphatics, as Mr. White ſuppoſes, contain all the additional matter of the limb in caſes of Phlegmatia *dolens*, they muſt, I conceive, become as much diſtended as varicoſe veins are, which, we know, become extremely enlarged, without adding much to the thickneſs of the inferior extremities. To the third query he anſwers, "he can trace the gradual increaſe of the lymphatics, in every inſtance, from the groin to the ends of the toes." The increaſe of the limb from above, downwads, may be traced; but I much doubt, whether in any inſtance of this diſeaſe, the lymphatics have been traced, gradually increaſing in this direction; for I cannot admit, that the mere enlargement of the limb proves the gradual increaſe of the lymphatics; much leſs can I allow, that the lymphatics are ever ſo far diſtended as to contain the quantity of matter depoſited and accumulated in the limb. I am of opinion, if the lymphatics of the limb of a dog, or any other animal, be tied, and a conſiderable intumeſcence of the limb take place, in conſequence of the interruption given to the courſe of the lymph, that the cellular membrane of the leg will be found much loaded with the exhaled fluid.

My *fourth* objection is, "*It does not appear, that in any of the fourteen caſes particularly related by Mr. White, or in any of the fourteen communicated to him, an enlargement of the lymphatic veſſels was at any period obſerved.*" This has not been farther noticed by Mr. White than in his anſwer to the foregoing objection.

From what has been alleged, I am led to conclude, 1ſt. that conſiderable obſtruction, and evident diſtention of the lymphatic veſſels do frequently occur, without producing Phlegmatia *dolens*; 2dly. that this diſeaſe very frequently occurs without any perceptible enlargement of the lymphatic veſſels; and, 3dly. that if the lymphatics, in certain caſes of Phlegmatia *dolens*, have been, or ſhall be found increaſed in ſize, this is rather

rather to be confidered as an accidental, than as an effential circumftance, or as a confequence rather than as a caufe of the difeafe.

III. *Mr. White's Objections to the Theory of Phlegmatia Dolens, given in my Essay, considered.*

As my Theory is given very fully, and with its illuftration, occupies no lefs than eleven pages, I muft beg leave to refer the reader to my Effay, and fhall only ftate in this place, that I confider the proximate caufe of the difeafe, as confifting in *an inflammatory affection, producing fuddenly a confiderable effufion of ferum and coagulating lymph from the exhalents into the cellular membrane of the parts affected.* To this theory Mr. White has raifed various objections, in the Second Part of his *Inquiry*, p. 32, &c. which I fhall now proceed to examine.

He begins with faying, " There can be no inflammation in any part of a human or animal body, wherein red blood is ufually circulated, but it will fhew itfelf by an increafed colour of the part, either of a red, purple, or bluifh tint. Red blood is not ufually circulated in the cornea, the pleura, nor the peritonæum; but when thefe parts are inflamed, a rednefs frequently takes place, in confequence of fome red blood in them. There can be no inflammation in the external parts of mufcles, cellular membrane, or any part of the fkin; in the fuperficial blood veffels, cutaneous lymphatics, in their fuperficial trunks, or in the glands of the leg and thigh; but it may, in every inftance where it occurs, be feen in every living human fubject." Mr. White may properly be afked here, what kind of affection takes place in the limb of a perfon afflicted with acute rheumatifm, when there is pain, tendernefs, rigidity, tumour, and heat, without any red, blue, or purplifh tint to be feen? or, what is the affection of a conglobate gland, which occurs in confequence of a chancre, and is attended with pain, heat, and enlargement, without any difcolouration of the fuperincumbent fkin? It is to be remembered, that the cutis is not tranfparent, and confequently cannot indicate the difcolouration of the fubjacent inflamed parts, except when the inflammation has extended to its upper furface. Though no external rednefs appears in the two cafes mentioned above, there is, I conceive, an inflammatory affection of the mufcles and ligament, &c. in the former cafe, and of a lymphatic in the latter cafe; and, I have no doubt, but this would be fhewn by diffection. Neither have I any doubt, that a limb, affected with Phlegmatia *dolens*, if diffected in the beginning of the difeafe, would exhibit unequivocal

vocal marks of inflammation. I think I have accounted ſatisfactorily for the outer ſurface of the ſkin not being reddened in this complaint.* That Mr. W. has expreſſly admitted the preſence of inflammation in Ph. *dolens*, will now be ſhown from his own words. In the former part of his *Inquiry*, &c. where he is treating of the cure of this malady, he very properly obſerves, that it muſt vary according to the different ſtages; and adds, "The firſt, which may be called the *inflammatory*, muſt be treated in the antiphlogiſtic method; but as this *inflammation* is not the original diſeaſe, but a *ſymptom* only, occaſioned by the diſtenſion of the lymphatic veſſels and glands, it is not neceſſary or prudent to waſte the patient's ſtrength by large evacuations," p. 59. The point then, properly at iſſue betwixt Mr. White and me, is not, whether there be an inflammatory affection of the limb and other parts, but whether the inflammation be the primary complaint, or only a ſymptom ariſing from diſtenſion? To determine this point, let us firſt advert to what takes place in the lower extremities, when affected with anaſarca, and afterwards with Phlogoſis *erythema*. In this caſe the anaſarca is very properly conſidered as protopathic, and P. *erythema* as deuteropathic; becauſe the pain, heat, and redneſs, &c. characterizing the latter diſeaſe, are ſubſequent to the ſymptoms which mark the preſence of anaſarca, and evidently originate from the irritation produced by the diſtenſion of the ſkin. On the contrary, in Phlegmatia *dolens*, the pain heat, tenderneſs, &c. precede the intumeſcence, and are relieved by it; conſequently, the inflammatory affection muſt be the primary one, and not a ſymptom only, as Mr. W. has here aſſumed. When Ph. *erythema* occurs as a ſymptomatic affection, it is neceſſary that it be marked by the ſame concourſe or aſſemblage of ſymptoms, as when it occurs as a primary affection, and the ſame may be ſaid of other ſymptomatic affections: Therefore, if inflammation occur as a ſymptom of Phlegmatia *dolens*, it muſt be marked by the ſame concourſe of ſymptoms which conſtitute inflammation, when occurring as an original diſeaſe. Now Mr. White has actually ſtated, that there is inflammation in Phlegmatia *dolens*; and as he has alſo ſtated, that the ſkin is whiter than natural in this diſeaſe, he muſt have formed an idea of inflammation which does *not* "ſhew itſelf by an increaſed colour of the part, either of a red, purple, or bluiſh tint;" and admitting the exiſtence of inflammation in this diſeaſe, he could not well give it a ſeat different from what I have done in my Eſſay.

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* See Eſſay on Phlegmatia Dolens, p. 209, &c.

Pyrexia *preceding* or accompanying the complaint, "continues Mr. White," is not an unerring ſymptom of inflammation, either acute or chronic, by ſome called paſſive, eſpecially when there is no hard, ſtrong, full pulſe, no redneſs, itching, nor throbbing of the part, no plethoric ſtate of the general ſyſtem, nor any partial plethora, which are none of them *common attendants* upon this diſeaſe. "It would appear then from this conceſſion alſo, that ſymptoms of inflammation are *occasional*, if not *common* attendants on this diſorder.

"The excruciating pains, ſtiffneſs, tenderneſs, increaſed heat, and ſwelling of the parts may," according to Mr. White, "be accounted for on other principles." I anſwer, not ſatiſfactorily; for unleſs he can make it appear that effects precede their cauſes, his own explanation muſt be rejected.

The ingenious author next chuſes to employ the interrogatory mode of attack. He puts no fewer than twelve queſtions in immediate ſucceſſion, all of which I ſhall now conſider, in the order he has placed them.

Question 1. "If the *lymph* coagulate in the cellular membrane, why does this happen in the beginning, rather than towards the termination of the diſorder?" *Answer.* By *lymph*, properly ſpeaking, is underſtood a fluid that is, or has been, contained in the lymphatics or abſorbents. Now I have no where ſaid, that the *lymph*, taken in this ſenſe, does coagulate in the cellular membrane of a limb affected with Ph. *dolens.* I maintain, that in addition to the exhaling fluid, uſually poured into the cellular membrane, a quantity of *coagulating lymph*, or *fibrine*, according to the new chemical nomenclature, is thrown from the exhalants into the cellular membrane of the affected parts, and there coagulates in the beginning of this diſeaſe; but that afterwards, only the common exhaling fluid, or even a more watery humour, is thrown into the cellular membrane. If Mr. White wiſhes to know, why fibrine is effuſed in the beginning rather than towards the termination of the diſorder, I reply in his own words, that in the firſt ſtage there is inflammation, and that in the third or laſt ſtage, "pain and fever have entirely left the patient, and no complaint remains except the ſwelling of the limb, and perhaps a *general relaxation.*" *Inquiry*, &c. Part I. p. 64. Every perſon acquainted with the practice of phyſic knows, that when the membranes of cavities are inflamed, fibrine or coagulating lymph is thrown into theſe cavities by the exhalants, as in Peritonitis *puerperalis*, Phlegmatia *dolens*, &c. and that a thin watery fluid is poured into them by the exhalants, as in aſcites, anaſarca, &c. when theſe veſſels are in a relaxed ſtate.

Question

Question 2. "If the lymph is contained in the cells of the cellular membrane, if we obtain one drop by puncture, why is the quantity limited?" *Answer.* As the fibrine and other fluids, effused into the cellular membrane, coagulate; if a puncture be made through the skin in the first stage of the complaint, only a small quantity of serum, or serosity, issues from it in general: But the quantity of watery fluid which is thus discharged in a more advanced stage of the disease, is greater, because the coagulated and fluid parts are then more distinct, and the proportion of liquid to the coagulum is also increased, owing to the effusion of fibrine ceasing, whilst that of the exhaled fluid continues, or is increased. It is probable that adhesion takes place in consequence of the inflammation, and that the communication betwixt the different parts of the cellular membrane is thence rendered less free than natural.

Question 3. "Why is its absorption not promoted by the common means." I have yet to learn that it is not. I think it very probable, that the absorbents take up and transmit more exhaled fluid throughout the whole course of this disease, to the thoracic duct, than in the natural state of the parts affected, and that the intumescence is produced and kept up by a materially increased effusion from the inflamed exhalants in the first stage, and from the relaxed exhalants in the last stage of this complaint.

Question 4. "If the coagulating lymph be thrown out in consequence of a greater determination of blood, why does it not take place more early?" *Answer.* Mr. White's cases shew, that the disease takes place as early as in twenty-four hours after delivery;* that it also occurs at the end of the first, second, third, and fourth week, and as late as five weeks†. This is just what we should expect, from considering the disease as an inflammatory affection, depending upon a greater determination of blood to the parts, and occasioning an effusion of coagulating lymph, or fibrine, into the cellular membrane. If the exciting causes be early applied, the disease will appear early; if late, the disease will appear late. For further particulars relative to the remote causes of the disease, I must beg leave to refer to my Essay.

Question 5. "Why does not the same occurrence sometimes take place after tapping for the dropsy of the ovarium, amputation, or the operation for the popliteal aneurism?" *Answer.* I

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* See the case of Betty Grime. *Inquiry*, Part 1. p. 33.

† Ibid. p. 9.

dare not ſay, that the diſeaſe does not ſometimes take place after theſe operations. If Ph. *dolens* were capable of being produced by injuries of the lymphatics, it ſhould frequently ſucceed to the operation for the popliteal aneuriſm, becauſe the deep ſeated lymphatics which accompany the femoral artery muſt frequently, if not always, be divided or tied in this operation. Why the diſeaſe occurs moſt frequently in lying-in women, I have endeavoured to explain in my Eſſay.

Question 6. Why does it ever happen to women during pregnancy, when an oppoſite ſtate muſt prevail?" *Anſwer*. What Mr. White means by an *oppoſite ſtate*, I do not, perhaps, exactly comprehend. He ſurely will not contend, that pregnant women are exempt from inflammatory complaints.—He here indirectly admits, that the diſeaſe does occur during pregnancy; an admiſſion, which at once ſhews the impropriety of the name he has given it, and the improbability of his theory, as far as relates to the production of Phlegmatia dolens. That the diſeaſe does take place during pregnancy is alſo proved by the caſes related by Dr. Evans and Puzos; and I have lately been made acquainted with a caſe, the ſubject of which was a lady, who was neither pregnant nor lying-in, nor had received any external injury, at leaſt this laſt circumſtance is not intimated to have happened. The caſe of Mr. R. related by Mr. White, appears to have been a caſe of this diſeaſe, and I underſtand from Mr. White, that Mr. Eaton, who attended this gentleman with him, conſiders it as ſuch. Now, if this were actually a caſe of Phlegmatia dolens, it muſt be admitted as proving that the diſeaſe is not excluſively confined to the female ſex, and as ſhewing further the impropriety of naming it *Phlegmatia alba dolens puerperarum*. That there was a fracture of the pelvis is not improbable from Mr. White's account of the caſe; but as he did not aſcertain the ſituation of the fracture, it cannot, without further evidence, be admitted, that the fracture was ſituated where Mr. White ſuppoſes it to have been from the view of a ſkeleton, much leſs that the lymphatic veſſels, which deſcend into the tubular part of the pelvis, were ruptured or lacerated by the fractured os pubis. He aſks, "How can we poſſibly account for the ſwelling of *the left ſide of the ſcrotum only* at the expiration of a fortnight, but from the rupture of the lymphatic veſſels of that ſide of the ſcrotum, and the mouths of thoſe veſſels healing in that time and *totally* obſtructing the paſſage of the lymph?" Another perſon may aſk, how can it be accounted for in this way, ſince the ſuperficial lymphatics of one ſide of the ſcrotum anaſtomoſe with thoſe of the other ſide, and ſince the ſuperficial lymphatics of one ſide anaſtomoſe with the deeper ſeated lymphatics of the ſame ſide, and theſe laſt paſs

with

with the lymphatics of the teſticle into the abdomen and not under Poupart's ligament? Beſides Mr. W. has declared, that there was not "any ſwelling or inflammation in the glands," p. 121, and he makes no mention of having traced or obſerved any enlargement of the abſorbents of the ſcrotum; yet, according to his theory of the diſeaſe, both the inguinal glands and the lymphatic trunks ſhould have been enlarged in this caſe. It is remarkable that Mr. White makes the following poſitive aſſertion relative to the caſe of Mr. R. after ſaying that nothing can be *more ſimilar* than this caſe is to the *Phlegmatia alba dolens Puerperarum*: "Here was the ſame kind of ſwelling confined to the inguinal, hypogaſtric, and lumbar regions, and the whole limb and genitals on one ſide. The lymphatic veſſels *were ruptured in the ſame place and by the ſharp edges of the ſame bone*, with this difference only, that in one it is ſharp by nature, in the other it was become ſo by being fractured," p. 123: And afterwards, conſcious, as it would ſeem, that he was ignorant whether this were the caſe or not, he ſays, "*If it be granted me that this ſwelling was occaſioned by the broken bone* injuring the lymphatics, how can we account for it upon any other principle than that the lymphatic veſſels, which paſs over and wrap round the body of the os pubis, were ruptured by the ſharp edges of that broken bone, the mouths of which were healed up and rendered impervious?" p. 124.—He afterwards ſays, "If it had proceeded from inflammation only, it would ſcarcely have continued ſo long as ſeveral weeks," p. 125.—For my own part, I cannot perceive that any argument worth attention can be drawn from the continuance of this ſwelling for ſeveral weeks againſt its having proceeded from inflammation. And upon what ground will Mr. W. deny that this affection of Mr. R's limb, &c. was an inflammatory affection beginning in the injured parts, extending down the limb, and accompanied by an effuſion of fibrine and other fluids into the cellular membrane? Mr. White has ſtated, in the quotation given above, that the ſwelling was confined to the inguinal, hypogaſtric and lumbar regions, and *the whole limb and genitals on one ſide.*—A queſtion may therefore ariſe, whether one half of the penis was affected in this caſe? he informs us that neither of the teſticles were. Another queſtion may alſo ariſe, reſpecting the parts affected in women labouring under Phlegmatia *dolens*, viz. whether this diſeaſe affected only one of the nymphæ and one ſide of the clitoris, meatus urinarius, and vagina, in thoſe caſes wherein Mr. White found only one labium vulvæ affected?

After this digreſſion I ſhall now return to the remaining queſtions propoſed by Mr. White.

Question 7. "Why does it generally only take place in one limb,

limb, or in the other when the firſt is recovering?" *Anſwer.* In inflammatory diſeaſes it very frequently happens that only one ſide or part of the body is affected; thus, for example, in Peripneumonia *Pleuritis*, one ſide of the thorax only is affected; and we very frequently find one teſticle, one eye, one ear, one kidney, one breaſt, &c. only inflamed. If Mr. White can account for this, he will alſo be able to account for one limb being affected in Plegmatia dolens. If the lymphatics be divided on both ſides during labour, why does not the diſeaſe occur in both ſides at the ſame time?

Question 8. "Why the lymphatics of one ſide only ſhould be filled is evident; but why the blood-veſſels of one ſide ſhould have the inflammation confined to them, when the ſame cauſes have been applied to both, and they communicate with each other, is not ſo obvious?" I wiſh to know what reaſon Mr. White has for ſaying, that *the same causes* have been applied to the blood-veſſels on both ſides, when Phlegmatia *dolens* only takes place in one? There muſt, I conceive, be a difference with reſpect to the cauſes, or the ſame effects would follow in both limbs, &c. That the blood-veſſels communicate by means of anaſtomoſes is true, and we have Mr. White's authority for ſaying that the lymphatics "are known to be *more numerous* and to anaſtomoſe *more frequently* than the arteries, p. 73.—He agrees even with Mr. Cruikſhank, that the ſuperficial lymphatics in the extremities of the human body are very numerous, "fourteen trunks frequently accompanying one vein." *Inquiry, &c.* p. 81. Hence it will appear from Mr. White's own words, that the lymphatics communicate more frequently with each other than either the arteries or veins. How then is it *ſo eaſy* to conceive that the lymphatics of one ſide only may be affected, and *ſo difficult* to conceive that the blood veſſels of one ſide only ſhould be affected? Again, does not inflammation frequently attack one inferior extremity without affecting the other? Does it not ſeize the leg without affecting the thigh of the ſame ſide? Does it not frequently affect the lower part of the leg without extending to the upper part of the ſame leg? If Mr. White can account for theſe limited inflammatory affections, I preſume he will be able to account for Phlegmatia *dolens* being, in general, more or leſs confined to one ſide, yet ſometimes extending to both.

Question 9. "If it proceeds from a general cauſe, why are its limits ſo certainly defined? *Anſwer.* The limits of the diſeaſe, as I have already proved, are not ſo exactly and certainly defined as Mr. White has been pleaſed to ſtate. It may not be improper to adduce here Levret's account of the manner in which the diſeaſe paſſes from one ſide to the other.—"Mais ſi,

dès

dès le commencement de la diminution de la cuiffe, les fueurs ne fe declarent pas, & que les urines, ou les felles ne deviennent pas plus abondantes & laiteufes, il faut s'attendre que l'humeur ne fait que fe déplacer, & qu'elle fe dépofera bientôt fur quelqu' autre partie. En effet, fa marche la plus ordinaire, en pareil cas, eft de paffer de la cuiffe à la feffe du même côté; elle gagne enfuite le dedans du baffin, puis la feffe & la cuiffe du côté oppofé & de-là fe communique à la jambe & au pied; enforte que ces différentes parties éprouvent fucceffivement les mêmes fymptômes qu'on avoit remarqués dans la premiere extrémité." *L'Art des Accouch.* §§. 941 & 942. How perfectly this cafe may be explained by confidering it as a gradual extenfion of the inflammatory affection from one limb to the other! How unfatisfactory muft every attempt to account for it be, agreeably to Mr. White's hypothefis!

Question 10. "Why does it attack the lumbar region, where the determination of blood before delivery would be greater than after? *Anfwer.* I muft beg leave to refer Mr. White to my Effay, p. 202, and p. 264, &c. for the reafon why Phlegmatia *dolens* frequently attacks the lumbar region; and I cannot allow, that the determination of blood to the lumbar region is greater before delivery than after. By lumbar region, I mean here thofe parts of the loins that are affected in Phlegmatia *dolens*, not including thofe abdominal vifcera, which may be faid to be fituated during pregnancy in the lumbar region.

Question 11. "Why does it commence at the upper part, rather than at all points alike?" *Answer.* There is, I believe, no part of the lower extremity, in which this difeafe may not, or has not begun, and this correfponds perfectly with what ought to be expected from an inflammatory affection. Why it moft frequently begins in the lower part of the trunk, I think I have affigned fatisfactory reafons in my Effay. If a fwelling invade one of the lower extremities from mere obftruction of the lymphatics at the brim of the pelvis, I fhould expect it to begin fometimes at leaft at the foot, and to proceed gradually up the limb; to be of the œdematofe kind, but rather more firm than œdematofe affections generally are when occurring in debilitated habits, and to be attended with little pain, and that pain fucceeding to, not preceding the diftenfion of the fkin. In fhort, I fhould expect the intumefcence to refemble Phlegmatia *diuturna* more in its fymptoms than Phlegmatia *dolens*.

Question 12. "If this diforder be occafioned by a general or local plethora, or there exifts a general or local inflammation; why does it ever happen to tender delicate women, exhaufted and debilitated by violent floodings and other evacuations? *Answer.* There is, perhaps, no ftate of the fyftem, however

however great the debility, in which inflammation may not take place. We find lying-in women, much weakened by floodings and other evacuations, not uncommonly attacked with Peritonitis *puerperalis*. And it has been ſhewn above, that Mr. White himſelf ſays, there is inflammation in this diſeaſe, and that the firſt ſtage may be named inflammatory, and that he propoſes the antiphlogiſtic mode of treating it. The puerperal ſtate, which is characterized by debility and irritability, and a ſudden change in the determination of the circulating fluids, appears to give a peculiar prediſpoſition to Phlegmatia *dolens*, and to occaſion the inflammatory affection of the lower extremity, &c. to be attended with an extenſive effuſion of fibrine and other fluids into the cellular texture, and to exhibit that concourſe of ſymptoms which conſtitutes the character of the diſeaſe under conſideration.

In page 133 of my Eſſay on Phlegmatia *dolens*, I have ſaid, "It has in many inſtances attacked women who were recovering from puerperal fever, and in ſome caſes has ſupervened, or ſucceeded to thoracic inflammation." Mr. White, in commenting upon this paſſage, makes the following declaration: "That anaſarca, puerperal fever, thoracic inflammation, and *Phlegmatia alba dolens puerperarum* may ſucceed each other, or even exiſt together, I have no reaſon to doubt, as they are by no means incompatible; but as I have never ſeen ſuch occurrences, I ſhould therefore conclude, they are ſo rarely met with, as not to be ſufficient to build a theory upon." *Inquiry*, p. 38.

I have ſufficiently proved the truth of my obſervation by caſes which have occurred to myſelf and others, and I ſhall beg leave to adduce here, two extracts from Mr. White's Inquiry, &c. p. 1, and then leave the reader to determine how far the above aſſertion is ſupported or contradicted by his own caſes. Speaking of Martha Wilkinſon, (Caſe vii. p. 22,) he ſays: "On the ninth day after delivery, ſhe was ſeized with a *pleuritic stitch* on her right ſide; on the thirteenth ſhe was attacked with pain in her left groin, and labium pudendi, &c." Speaking of Elizabeth Rothwell, (Caſe viii. p. 23 and 24,) he ſays: "She was kept very warm, never went out of her room, nor did any thing, by which ſhe could poſſibly catch cold, till twenty days after her delivery, when *a window was opened in the room* about a minute or two. *In an hour after she was seized with a pain in her right side* and ſhoulder, but had no cough. *She was bled, which eased the pain.* Two days after, ſhe was attacked with pain in the groin, labium pudendi, &c."

To conclude, although I have laboured ſtrenuouſly to ſhew, 1ſt. That the affection of the labium pudendi is improperly conſidered

ſidered as the pathognomonic ſymptom of Phlegmatia *dolens* by Mr. White; 2. That my caſes which, he has endeavoured to reject, were true and genuine caſes of the diſeaſe; 3. That his theory is purely hypothetical; and, 4. That the objections which he has raiſed to my theory, have little, weight. I hope I have no where departed from that reſpect which is due to a gentleman of Mr. White's age, experience, rank, and abilities, as a writer and practitioner, and who may juſtly be conſidered as having deſerved well of his profeſſion and his country for more than half a century.

To the Editors of the Medical and Phyſical Journal.

Gentlemen,

FROM the readineſs with which every hint at improvement is received into your valuable Journal, I am induced to ſend you the following remarks on an important diſorder, at preſent but imperfectly known, not with a view to promulgate any new diſcovery, but to ſolicit the aſſiſtance and information of thoſe whoſe obſervations have been more extenſive. The hiſtory of the following diſorder, although not wholly novel, has but ſeldom engaged the attention of practitioners; and but a ſmall number of thoſe, who have attentively watched its riſe and influence, have found it deviate from its accuſtomed fatality. According to the variety of ſymptoms that have been diſcovered, it has obtained various denominations, as Hæmorrhœa Petechialis, Petechiæ ſine Febre, and Petechianoſis; but as no accurate pathology has yet been developed, I am inclined to think the petechiæ ſine febre an appropriate appellation.

Authors have not unfrequently deſcribed it as merely ſymptomatic, and in general ariſing from hæmorrhage. Dr. Cullen ſays, "*The petechia has been, by all our noſologiſts, enumerated amongſt the exanthemata; but as, according to the opinion of moſt phyſicians, it is very juſtly held to be always a ſymptomatic affection only, I cannot give it a place.*" As its appearance has, of late years, been more frequent than uſual, and its moſt probable origin traced with ſome degree of accuracy, it has been found chiefly to reſult from univerſal debility, and a general tendency to putrefaction; and that hæmorrhage, ſo far from being a regular diagnoſis of the diſeaſe, has only ſupervened as the petechiæ have accumulated.

 The

The first instance that came within my observation was a child four years of age, who had laboured under the complaint about a fortnight. At this time the petechiæ were pretty numerous; the child was excessively languid; appetite impaired; the eyes sunk in the head, and some small degree of pyrexia. I had previously been informed, by a medical friend in this neighbourhood, that the *coccinella septem punctata*, if administered in scruple doses, had a salutary influence in this complaint; and being well convinced, that unless some speedy relief could be obtained, the child would inevitably die, I ventured to prescribe a mixture, with two drachms of cochineal, two drachms of aromatic confection, filled up with the decoct. cort. This was taken in the dose of two table spoons full, three times a day, and regularly persisted in during the space of a fortnight, at the expiration of which time, having taken six drachms of cochineal, I had the pleasure to find my little patient restored to perfect convalescence. During the progress of the disorder, there was little or no tendency to hæmorrhage, and the pulse between 80 and 95; the legs were frequently swelled at night.

One circumstance, perhaps, it will be necessary to mention, the child was very much troubled with scorbutic eruptions on the face, hands, and arms, for a considerable time previous to the appearance of petechiæ. A generous diet was insisted on, and the child partook of animal food and red wine.

The next case that occurred was the daughter of Mr. J——, aged five, of a very delicate habit; this little girl had been poorly for a considerable time before the petechiæ appeared; the appetite was exceedingly small, and the whole body much wasted: The tongue assumed a very dark appearance, and, in fact, there was every reason to believe putrefaction would shortly take place. Willing to try if the sole use of the coccinella (considered as a medicine) would have any salutary effect, I prescribed for the child, powders, containing a scruple in each, and directed one to be taken three or four times a day, in a glass of port wine. They were regularly continued, and in the space of ten or a dozen days the petechiæ began to disappear, and her appetite improved considerably. In this instance there was no tendency to hæmorrhage, nor any distressing symptom but what evidently arose from accumulated excitability: the pulse varied often, and flushing heats alternately suffused the face; but in proportion as the vital powers increased in energy, by the cautious exhibition of stimuli, the flushing heats were removed, the appetite increased, and the petechiæ gradually disappeared; from which we may reasonably infer, that the complaint, in this instance at least, owed its existence to defective excitement.

The

The third and laſt caſe was Mrs. G——, ætat. forty; pale complexion; ſubject to pulmonic complaints, and perpetual dyſpnœa; the legs had been œdematous for ſome time before the petechiæ appeared. She had accuſtomed herſelf to low living, drinking principally water, and eating ſparingly of animal food; pulſe ſcarcely perceptible, and ſhe was peculiarly ſubject to ſyncope. The petechiæ were not numerous, and there appeared ſome large diſcolourations of the ſkin, more reſembling vibices than petechiæ. She was not confined to her room, and occaſionally viſited her friends, but complained of much pain after exerciſe; the petechiæ were of a very bright ſhining purple.

It will be needleſs to particularize every trivial incident. She perſevered in the uſe of coccinella and wine; and the vibices firſt diſappeared, after them the petechiæ. It is now ſome months ſince they left her; and, excepting a few pulmonic ſymptoms, totally abſtracted from the diſeaſe in queſtion, ſhe is tolerably well. How far the influence of coccinella extends in the cure of theſe eruptions, or whether the recovery is to be attributed to the uſe of the wine, I cannot undertake to determine. Of their ſtimulative effects, I cannot ſay I obſerved any demonſtration; probably they may poſſeſs ſome antiputreſcent influence, or a peculiar ſpecific property, that is eſſentially beneficial in the diſorder above mentioned. I have peruſed many narratives, and have generally found their appearance deemed indicative of diſſolution, notwithſtanding the liberal uſe of antiſeptics, fruit, wine, &c. By ſome they are ſuppoſed to ariſe from ſcurvy; probably this may, in part, accelerate their appearance, and if ſo, mineral alteratives, or medicines that determine to the ſkin, may have their ſhare of efficacy. In general, however, the conſtitution will be found ſo much impaired, as to admit of nothing but the tonic plan. It is to be wiſhed, that ſome practitioner, whoſe enquiries have been particularly directed to this dangerous malady, may develope his reſearches, and throw additional light on a ſubject, at preſent but imperfectly underſtood.

I am, &c.

J. H. GROSE.

Winslow, July 22, 1801.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

IF the incloſed caſe, and obſervations thereon, ſhall be found to contain information deſerving a place in the Medical and Phyſical Journal, it is much at your ſervice, and the inſertion will greatly oblige,

Yours, &c.

CHRIST. DENNETT, Surgeon.

Soho Square, July 28, 1801.

IN the preſent ſtate of Vaccine Inoculation, every caſe which deviates in any degree from the regular progreſs, either under particular circumſtances or in appearances, ought to be noticed. The following, in my opinion, is one of theſe.

On the 9th of November laſt I was called to viſit the child of Mr. P——d, who I found had got the natural Small-pox of the diſtinct kind, which had made its appearance on the evening of the 7th. As there was a brother who had not had the diſorder, I thought this would be a proper trial of the preventive effect of the Cow-pox; accordingly I immediately inoculated him in the left arm with Vaccine matter I had by me, taken on the 4th of the month.— he child was about twelve months old. On the 11th, there was a ſlight degree of inflammation, but very pale. On the 12th, it had quite diſappeared. I this day inoculated him again in the right arm, with recent virus, taken half an hour before; this had the deſired effect. On the 15th, it looked red, and might be diſtinctly felt, but no inflammation attended the firſt inciſions. On the 17th, the right arm loo ed very florid; there was not the ſlighteſt appearance of inflammation on the left, but the inciſions might be ſeen. I was informed that the child had two puſtules in its face; on examination, it appeared to me only a violent humour from teething, for the ſurface was flat and as large as a ſilver penny, attended with ſeveral inflamed pimples about the ſize of a ſmall pin's head. The left parotid gland was alſo much enlarged, which the mother attributed to cold. On the 18th, the right arm was inflamed more than the day before, but I did not examine the left. On the 19th, I underſtood the child had paſſed a very reſtleſs night, and found he had then conſiderable fever. The inflammation on the right arm extended as large as a half crown. The mother greatly ſurpriſed me when ſhe

ſaid

faid that the left arm had now began to inflame a little; this I found to be the cafe, attended with a fmall puftule in the centre of the incifions, refembling the Small-pox. I this day took matter from the right arm for preferving. 20th, The child had paffed a better night, but ftill feverifh; the inflammation going off both arms; the humour in the face much the fame. I this day took matter (on a lancet) for a friend, from the right arm, but it failed. I afterwards found he had inoculated the fame child twice before, without effect. On the 21ft, the febrile fymptoms nearly fubfided, which have been much more fevere than in any patient I have before inoculated with the Vaccine virus. On the 22d, the inflammation nearly gone; the veficle on the right arm getting dry; but on the left the puftule increafing; fever quite gone. On the 23d, the veficle on the right arm formed into a dry brown cruft; the puftule on the left had every appearance of being variolous, of which I informed my affiftant, and requefted that he would vifit the child in the afternoon, to obferve the fimilarity; and at the fame time take matter from the puftule. He did not think it fufficiently mature, however he took a little. The fpots on the face were dry, and had much the appearance of the right arm, except being more flat. On the 24th, I took matter from the puftule on the left arm. From this day the child required no further attendance.

With the matter taken from this child I have continued, in fucceffion, inoculating patients to the prefent period. That taken from the right arm has produced, in all, the true Vaccine diforder, in its moft mild form; and that taken from the left produces the Variolous.

This cafe, with many others, particularly thofe attended with eruption, inoculated by Dr. Woodville at the Small-pox Hofpital, and the one ftated by Mr. Malim, of Carey Street, is decifive, that expofure to variolous contagion at the time of the inoculation for the Cow-pox is highly improper, and fhould be a caution to every practitioner, when he has reafon to fuppofe the patient has been expofed to variolous effluvia; for although only local inflammation and veficle be produced from fuch inoculation, (as in this cafe) yet much difcredit may be brought againft the practice, if great circumfpection is not ufed: but, at the fame time, I think no one who has been much accuftomed to the Small-pox, can be deceived in the appearance.—This child, I conclude, received the infection from his brother during the eruptive fever, which counteracted the firft inoculation fo far as only to produce a fimple puftule.

As it muft be a defirable confideration to the profeffion at large to be in poffeffion of a method for keeping the Vaccine matter

matter, so that it may not fail, I shall beg to lay before the numerous Readers of the Medical and Physical Journal the plan I have found succeed best, and the manner of inoculation. I have practised the same in the inoculation for the Small-pox twenty years, (having had every opportunity for observation, while living with my father, whose extensive practice has been well known throughout the county of Sussex upwards of forty years) and it has scarcely ever been found to fail; indeed such success has attended it, that I was about communicating the manner of my treatment to the public; but when I found the Vaccine inoculation so generally received, I declined it, and shall now wait the event of further experience in the Cow-pox inoculation; for there are few of my patients who will permit me to practice it as I could wish.

When I intend to preserve the Vaccine virus, I usually take it on the eighth day, and puncture the vesicle in two or three places with the point of a lancet, then wait half a minute or a minute until the pellucid matter oozes out, which I take on the end of a quill, *and on the convex side*, cut in the manner of a tooth-pick, but not quite so tapering, with the point somewhat rounded, and a little bent, to prevent the matter being rubbed off in passing it into another quill, which I cut strait at the larger end, taking care to have them fit well before I attempt to take the matter. In this manner I have kept Variolous matter good for years; and have used Vaccine, thus preserved, four, five, six, and in one instance near seven months, yet have never found it fail, nor attended with too violent inflammation, or other bad symptom.—I have now matter by me taken in November last, which, I doubt not, would produce the Vaccine disorder. In this state it may be sent to any part of the world with certainty of success. The manner of my inserting it is by making three or four incisions or scratches across the arm, (as described by Dr. Paterson, in the last number of the Medical and Physical Journal) about one-fifth of an inch long, and half a line apart: these ought to be made in the slightest manner possible, as no more blood should be produced than what may serve to moisten the matter, which is to be repeatedly rubbed over the incisions until all be dissolved. If it has not been taken more than a day or two, it will require very little trouble to insert; but if it has been kept any length of time, I then hold it over the vapour of hot water for half a minute or a minute.—The quills usually employed for pens I find too hard, therefore select what are called the large feathers.

The

The following is a correct Copy of the London Testimonial in Favour of Vaccine Inoculation.

MANY unfounded reports having been circulated, which have a tendency to prejudice the public againſt the Inoculation of the Cow-pox; we, the underſigned phyſicians and ſurgeons, think it our duty to declare our opinion, that thoſe perſons who have had the Cow-pox, are perfectly ſecure from the future infection of the Small-pox.

We alſo declare, that the inoculated Cow-pox is a much milder and ſafer diſeaſe than the inoculated Small-pox.

Since our laſt Report, the ſignatures of the following Phyſicians and Surgeons have been added.

Dr. Pitcairn
Dr. Latham
Dr. J. Mayo
Dr. Hunter
Dr. Pemberton
Dr. Frampton
Dr. Fearon
Dr. Cooke
Dr. William Hamilton
Dr. Pinckard
Dr. Elliot
Dr. Babington
Sir Walter Farquhar, Bart.
Dr. Yelloly
Dr. Clarke
Dr. Poignand
Dr. Maton
Dr. Hooper
Dr. Bancroft
Dr. Hawes
Dr. Haighton
Dr. Boys
Dr. Walſhman

SURGEONS.

Mr. Addington
Mr. Aikin
Mr. Anderſon
Mr. Andree
Mr. Bliſs
Mr. Brickenden
Mr. Thomas Brown
Mr. Bureau
Mr. James Burrows
Mr. Cairncroſs
Mr. Chamberlaine
Mr. Charlton
Mr. Clutterbuck
Mr. Coleman
Mr. Cribb
Mr. John Curtis
Mr. L. I. Curtis
Mr. William Curtis
Mr. Dale
Mr. John Davies
Mr. Douglas
Mr. Dowers
Mr. Dunn
Mr. Dyſon
Mr. Foot
Mr. Murray Forbes
Mr. William Forbes
Mr. Foſter
Mr. Gaitſkell
Mr. Gardner
Mr. Gib
Mr. Thomas Goodwin
Mr. Grenville
Mr. Griſtock

Mr.

Mr. Harris
Mr. Hayes
Mr. J. W. Hill
Mr. Hogben
Mr. Hole
Mr. Hollings
Mr. William Holt
Mr. Home
Mr. Jeaffrefon
Mr. Johnfon
Mr. Jordan
Mr. Key
Mr. David Lewis
Mr. Lewthwaite
Mr Luxmore
Mr. Maffie
Mr. Maule
Mr. Meffiter
Mr. Mofs
Mr. O'Connor
Mr. Owen
Mr. James Parkinfon
Mr. Paternofter
Mr. Perfect
Mr. Phillips
Mr. Platt
Mr. Porter
Mr. Ramfden
Mr. Reid
Mr. Ricardo
Mr. Seagram
Mr. Seares
Mr. Sharp
Mr. Thomas Sheldon
Mr Shirley
Mr. Simmonds
Mr. Edward Smith
Mr. Jofeph Steel
Mr. R. H. H. Steel
Mr. Strong
Mr. Turnbull
Mr. Tufon
Mr. Ware
Mr. William Warner
Mr. Witham
Mr. Wood
Mr. Woodward
Mr. Wye
Mr. G. W. Young
Mr. Jonathan Young

To the Editors of the Medical and Phyfical Journal.

GENTLEMEN,

I Was in hopes that fome of your learned correfpondents would take the trouble of offering to the younger part of your readers, fome Remarks on " An Analyfis of the Kilburn Mineral Waters, by John Blifs, Member of the Royal College of Surgeons in London," publifhed in the Medical Review for July, 1801: Having been difappointed in my expectations, I am induced to trouble you with this letter, left fuch an analyfis pafs entirely unnoticed.

Every perfon acquainted with analytical chemiftry, will readily admit the difficulty of determining, in the prefent ftate of our knowledge, with any degree of certainty, the abfolute quantity of the ingredients which enter into the compofition of any fubftance. This is true of concrete metallic and earthy fubftances

ſubſtances, but deliqueſcent ſalts, and mineral waters, are analyſed with ſtill greater difficulty. All authors agree on the impoſſibility of aſcertaining the preciſe quantity of the various matters ſuſpended or diſſolved in waters, on account of the ſmall portion contained in a given weight of the liquid, and on account of the nature of theſe matters. But, if we cannot arrive at ſuch ſatisfactory reſults as we could wiſh, we may generally diſcover pretty accurately, what ſubſtances do, and what do not, enter into their compoſition.,

It was certainly laudable in Mr. Bliſs, to undertake the experiments he has publiſhed; and for my own part, I am very thankful to ſuch gentlemen as employ their ſkill and abilities in promoting the ſtudy and knowledge of chemiſtry, perſuaded as I am, that it is a branch of ſcience too much neglected by, though indiſpenſably neceſſary, to medical men.*

But has not that gentleman been rather unfortunate in his analyſis? I do not mean to enter into a long criticiſm, nor to follow the author in all his ſteps. I cannot help however remarking, that the account appears to me rather confuſed, and far diſtant from the conciſeneſs and perſpicuity of the analyſes publiſhed by Meſſrs. Vauquelin, Klaproth, Prouſt, &c.

It appears to me alſo, that Mr. Bliſs employs very extraordinary expreſſions; for inſtance, what does he mean by "Strontian lime water?" Did he uſe a ſolution of the two earths in water? or does he mean to ſay, that he uſed a ſolution of Strontian in water, as we uſe ſometimes a ſolution of lime in water? In this caſe, Strontian water would have been ſufficient and more correct; the appellation "Strontian lime," is, to ſay no more, a barbariſm.

According to Mr. Bliſs, there exiſts in the Kilburn water, beſides other matters:

Carbonate of lime, and of magneſia.

Muriat of lime, of magneſia, and of ſoda.

Sulphat of lime, of magneſia, and of ſoda.

I had been taught that the muriat of lime was decompoſed by the ſulphat of ſoda and of magneſia; and having the ſingular advantage of aſſiſting a learned philoſopher† in his chemical experiments, I had often witneſſed ſuch a decompoſition; conſequently, I was greatly aſtoniſhed at the reſult of Mr. Bliſs's analyſis ſtated as above. After his poſitive aſſertion, I could not, at firſt, but diſtruſt my memory, and have recourſe to experiments; I therefore poured ſome ſulphat of ſoda into a ſolution

* I may, perhaps, enlarge on this ſubject at ſome future opportunity.

† Richard Chenevix, Eſq. F. R. S. and M. R. I. A.

of muriat of lime; the effect was, as indeed might have been expected, a copious precipitate of ſulphat of lime, the muriat of ſoda remaining in ſolution.

I then poured ſome ſulphat of magneſia into a ſolution of muriat of lime, and there again a copious precipitate of ſulphat of lime was produced. Both ſolutions being ſaturated the reſult was more ſtriking, and preſented what was formerly called *miraculum chemicum*, a white ſolid body reſulting from the mixture of the two liquids. I thought theſe two ſimple experiments quite ſufficient to prove the concluſions of Mr. Bliſs to be erroneous. I, however, conſulted my celebrated countryman, Prof. Fourcroy, and I read: "All the ſulphats, except the calcareous, are decompoſed by the muriat of lime, by double elective attraction. Sulphat of lime is formed and conſtantly precipitated from the ſolutions of theſe ſalts mixed together, and there remain in the ſupernatant liquor ſoluble muriats, the baſes of which differ, according to the ſpecies of ſulphats which have been decompoſed." Syſtème des Connoiſſances Chimiques, Tome iii. p. 194. Again, "Sulphat of magneſia is decompoſed by carbonat of lime; carbonat of magneſia and ſulphat of lime are formed. Sulphat of magneſia is decompoſed by muriat of lime; muriat of magneſia and ſulphate of lime are formed. Sulphat of ſoda is decompoſed by muriat of lime; muriat of ſoda and ſulphat of lime being formed. Id. Tome iv.

Treating of mineral waters, the learned Profeſſor obſerves, "It would be almoſt ſuperfluous to ſay, that ſome ſalts exclude one another, as the ſulphat of ſoda and of magneſia with the nitrat and muriat of lime, the calcareous ſalts with the carbonate of ſoda, &c." Id. Tome ix. p. 298.

I muſt conclude by ſaying, that, if Mr. Bliſs cannot reconcile his analyſis with theſe facts and quotations, we are warranted to conſider it as erroneous, ſince it is aſſerted that he found in the waters, ſalts which cannot poſſibly exiſt together.

Great Mary Le Bone Street, I am, &c.
August 14, 1801. F. N. VANDIER.

Remarks

Remarks on the History and Nature of the Dolor Faciei.

(Concluded from p. 158—164 of our laſt.)

3. *Exanthematous Acrimony.*

EXANTHEMATA have been obſerved in this diſeaſe either in the face itſelf or in ſome other part of the body. Mr. ANDRE obſerved miliary eruptions in the face, upon the diſappearance of which the pain began to ceaſe; but when they broke out again, the pain recurred with the former violence. BONNARD and LEGAVAN relate inſtances of herpetic eruptions on the arm having preceded the diſorder; and the German tranſlator of Dr. FOTHERGILL's works ſaw it appear after a retrograde itch: The pain diſcontinued when the itch broke out again, but as ſoon as it was removed by proper remedies, the pain immediately returned; and the diſeaſe laſted in this way from the nineteenth year of age to the twenty-ſeventh.

4. *Scrophulous acrimony.* MENOURET and SELLE, loc. cit. aſſign this as a cauſe of the diſeaſe in two inſtances.

5. *Cartarrhous acrimony.* VOGLER and PUJOL, loc. cit.

6. RHEUMATIC MATTER. It is not improbable that this is a moſt frequent cauſe of that painful affection, which we find likewiſe confirmed by the authority of experience and obſervation.

7. *Venereal poiſon.* M. SIEBOLD is not inclined to admit this as a cauſe of the above diſeaſe, becauſe the venereal pain is always ſeverer in the night than in the day, without intermiſſions, and ſeems to be more ſeated in the bones than in the muſcular and nervous parts. We have, beſides, no obſervation of the venereal poiſon being aſſigned as the cauſe of that pain, if we except Mr. WATON. (*Journal de Medicine*, 1793, *month of March.*) It has been, however, obſerved, that the diſeaſe was ſucceſsfully treated with mercurials; but this can by no means ſerve as an argument for the venereal nature of the diſorder.

8. *Preceding ſerous profluvia ſuppreſſed.* Dr. LENTIN relates a caſe in which a purulent ſalivation preceded the pain, by the appearance of which, the paroxyſm of the pain became leſs vehement and ſhorter: In another caſe the pain ſucceeded a running of the ears; and Dr. Thilenius ſaw a patient who had a kind of gonorrhœa previouſly to the pain, which began to run again as ſoon as the pain had ceaſed.

VAN SWIETEN (*Commentar. in Aphoriſm. Boerhaave*, §. 757) and DAVIDSON (*loc. citat.*) have obſerved a pain in the face ſimilar to an ague, which, however, eſſentially differs, ac-

cording to Dr. FOTHERGILL, from the diforder we are fpeaking of. The patient of VAN SWIETEN had a very fevere hemicrania, lafting every day for eight hours, which, after this time, abated by degrees. At the firft attack the pain appeared in the place where the branch of the fifth pair iffues from the foramen fupraorbitale: In another cafe a topical febrile pulfe was remarked at the affected place: The difeafe was cured by the bark. The typic character of this pain, and the termination of its paroxyfm, with a ftrong perfpiration, fufficiently diftinguifhes it from the true Dolor Faciei.

CASE I. A country woman, near forty, of a thin habit, had been feized with a very violent and unufual pain affecting the *canthus* of the mouth, when fhe was ftooping to cut grafs. Having been afflicted with it for nearly a month, fhe was fo much relieved by topical venæfection, and leeches applied behind the ears, that fhe could attend to her ufual bufinefs till the next fpring, but the pain never wholly went off. About this time, when fhe felt herfelf big with child, the pain returned with greater violence than ever; fhe had feveral teeth drawn out, but to no purpofe. However, when a few months of her pregnancy were elapfed, the pain ceafed entirely, and fhe was luckily delivered of a healthy boy. Four months after the lying-in, the pain attacked her for the third time with the utmoft violence, and continued in this way till fhe came under the care of Mr. SIEBOLD. Her ftrength and health were much impaired, and fhe had been treated during the whole time with gummi guaiacum, aconitum, and linimentum volatile, but without the leaft fuccefs. The feat of the pain was not far from the right angle of the mouth, and confined to fo fmall a fpot, that it could be covered with the top of a finger, though fometimes it reached down to the *maxilla inferior*. Notwithftanding this, the fpot was not at all changed, but perfectly fimilar to the other fide; the preffure with a finger caufed no pain, and only the glands of the cheek were a little fwollen. At the edge of the under lip, not far from the affected place, fhe had a fmall pea-like foft tumour, of a blueifh colour, in which, however, fhe felt not the leaft pain; fmall ulcerations were, befides, perceived at the inner fide of the lips, moft probably occafioned by the teeth biting the place during the paroxyfm: the molar teeth were, for the moft part, wanting. The pain never returned at a certain time, and lafted fometimes only a few minutes; but at another time, day and night, with equal violence, fo that the patient could not fleep for nearly three months. Speaking or chewing would immediately excite the pain. The mouth became dry during the paroxyfm, and likewife the nofe, and the eyes began to fhed tears as foon

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as the vehemence of the paroxyſm was over: The patient endeavoured to mitigate the violence of the pain by preſſing the tongue againſt the affected place, by rubbing it with linen, and by contortions of the mouth and face. The taſte remained unaltered, except that ſhe ſeemed now to like ſweet things better than ſour and ſalt victuals, of which ſhe had been very fond before: She became more coſtive when the diſeaſe got worſe. Her abdomen was ſometimes ſwelled, but without any pain: the urine of a pale colour. In her younger years ſhe had been much ſubject to tooth-ach and ſwellings of the cheeks. The plan adopted for her cure was the following, viz. milk diet with ſoft eggs, and the following medicine, which ſhe commenced on the 11th of October, viz. ℞. *Extract cicutæ*, drachm ij. *Pulv. herb. cicutæ* ʒß. *M. F. pill. pond.* gr. iij. S. four times a day, four pills. ℞. Decoct. cort. Peruviani unc. viij. gum arabic, dr. iij. ſyrup de althæa unc. Ms. to take every three hours half a tea cup full. As the pains, however, increaſed, and the pulſe became full and tenſe, ſhe diſcontinued the bark.

12th. The night very reſtleſs and painful; her pulſe is full and hard; ſkin ſoft and moiſt; no ſtool ſince three days, on which account ſhe took one drachm and a half of ſal polychreſtum.

13th. A hard ſtool; continued the pills, and a decoction of hemlock was ordered to be kept in the mouth for ſome time.

14th. Another doſe of the ſalt for the coſtiveneſs, and every two hours three pills.

15th. A tolerable night; a little ſleep; gentle evacuation of pituitous matter after adminiſtering an enema.

16th. The pain continued, on which account an emplaſtr. de cicut. was applied.

19th. The pain had conſiderably diminiſhed, and even ceaſed for three hours; coſtiveneſs; increaſed the doſe of the pills to one more; conſiderable abatement of the pain towards the evening.

20th. She had ſlept very quietly till midnight.

21ſt. Some pain when ſhe began to ſpeak; the ulcerations in the mouth, abovementioned, healed, ſkin moiſt; pulſe frequent, without being tenſe or hard; five pills at a time. She continued in this way the *cicuta* and the milk diet, till ſhe was attacked, *November* 14, with a febrile paroxyſm, accompanied with head-ache, vertigo, congeſtions of blood towards the head, and great anxiety: She perſpired at the ſame time ſo much, that ſhe was obliged to change her linen ſeveral times in the night. Theſe ſymptoms, however, were removed within four days, by bliſters and proper evacuants; but inſtead of them

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an abſceſs aroſe at the upper arm, attended with a copious ſuppuration; after which ſhe felt ſo much relief, that ſhe omitted rubbing the affected place; was able to ſpeak ſlowly, and to chew without exciting the pain.

December 13. From this date to the 21ſt of January ſhe took twice, daily, one ſcruple of the powder of valerian, which did her much good, the pains abating more and more of their violence, ſo that it was now thought proper to leave the reſt to Nature. On January the 25th, however, ſome ſevere paroxyſms of the pain returning, ſhe was ordered to rub in a mercurial ointment twice a day, which being continued for ſome days, the pain began to ceaſe, and ſhe felt nothing of it for two days, which continued ſo till ſhe was diſmiſſed, about the middle of February. The abſceſs had healed, but it was thought proper to make an iſſue near the place. Menſtruation had not appeared after the laſt pregnancy.

Case II.—The patient was a woman fifty years of age, of a robuſt habit, whoſe menſtruation had ceaſed twelve years ago. As a cook, ſhe had been much expoſed to the heat of the fire; her face was rather tender, but the ſkin relaxed. The pain firſt attacked her when ſhe was waſhing, and rubbing her face with a linen handkerchief; it came ſo ſudden, and with ſuch violence, that ſhe ſcreamed. It continued thus, day and night, for a whole year, but it never prevented her from ſleep: The pain extended itſelf over the whole right ſide, running down the temples and cheeks, and ſhe felt likewiſe a ſingular ſenſation on the forehead of that ſide, *(afflatio, ſuſurrus.)* She could open neither the lips nor the jaws, without exciting the moſt violent pains; but a conſiderable preſſure on the gums, or the external parts, did not produce the pain. She had ſuffered much tooth-ach from her younger years, on which account ſhe had all the teeth of the upper jaw drawn out. Several years previous to her being ſeized with the tooth-ach, ſhe was attacked with ſevere pains in the joints of the fingers, which ſhe ſuppoſed to have ceaſed by venæſection. Four years ago a particular eruption aroſe on the right arm, ſimilar to miliaria, and a year after ſhe had been ſeized with ſevere pains in the joint of the foot; the eruption appeared again a ſhort time before the *Dolor Faciei* came on. Theſe painful affections in the joints, and the miliary eruptions, ſeem to prove an affinity of this Dolor Faciei with the gout, particularly as we know, by the obſervations of Burserius and Strack, that topical miliary exanthemcta are frequently owing to arthritic matter.

Case III. A gentleman, of a full habit and ſtrong make, much ſubject to colds, had the misfortune of running his forehead againſt

againſt a tree, which immediately excited the *Dolor Faciei*, that laſted ſixteen years, extending to the noſe and down the cheeks. The pain was deſcribed by the patient as ſtinging, ſometimes as pinching: The affected ſide was ſomewhat decayed and emaciated. Though the patient had never had tooth-ach, yet he had ſome teeth drawn out, but without any effect. Moving the lips would immediately excite the pain; and, likewiſe, the leaſt draught of air frequently brought it on. A preſſure on the nervus infraorbitalis, at its exitus, would prevent the pain from extending itſelf over the face. After ſome time, during which he uſed the *Portland* powder, he experienced a conſiderable ſenſeleſsneſs in his hands, and ſometimes an inſupportable burning in the ſoles of the feet.

This caſe will be more fully deſcribed in future by Prof. *Iſenflamm*, of Erlangen, who has communicated a ſketch of it to our author.

Observations on the Efficacy of Olive Oil, for preventing and curing the Plague; by Dr. SCHEEL.

A Short time ſince, Mr. Baldwin, formerly Engliſh Conſul at Alexandria, diſcovered the great uſe of frictions with olive oil againſt the plague; on which ſubject he publiſhed a work, containing his method and obſervations at large. This book has been tranſlated into German by Dr. Scheel, who has added ſeveral remarks, of which the following is an abſtract.

" The effects of the olive oil, uſed according to the preſcription of Mr. BALDWIN, appear to be the following: 1. It prevents or diminiſhes at leaſt the abſorption of fluids of all kinds, by anointing the whole body, or by its not uniting with water, without the intervention of gum or alkaline ſalt. This circumſtance deſerves to be particularly regarded for explaining the efficacy of the oily frictions in preventing the infection. It is proved by experience and the evidence of facts, that humidity acts a great part in the different contagia, and eſpecially in the plague; for it has been obſerved, that the diſeaſe never penetrates into countries which are perfectly dry, or where the intenſe heat diſperſes all humidity, as in the interior of Africa; and the plague ceaſes in Egypt and Smyrna at the time of the hot and dry ſeaſon. During the time when the dry north-eaſt wind, called *Harmattan*, is blowing on the gold coaſt of Africa, every epidemical diſorder begins to ceaſe, and even the inoculation of the Small-pox is without effect. It is recorded by

by MURATORI, that in a very dreadful plague, by which *Venice* was depopulated in the 16th century, the great heat in the famous glaſs manufactory of *Murano*, preſerved all the workmen that were expoſed to it, from the infection of the plague. A certain degree of humidity is likewiſe required in the ſcented exhalations of bodies for being formed and conveyed to the organ of ſmell. Every thing beſides, that deprives the body of its natural unctuous covering, opens a way to infection, and MURATORI cautions us therefore very much againſt waſhing with ſoap in the time of a plague, becauſe it promotes infection and brings on the moſt dangerous conſequences.

"2. It prevents the action of atmoſpherical air on the body, and conſequently the oxydation of oxydible ſubſtances in the cutaneous veſſels, and obſtructs likewiſe the excretion of thoſe ſubſtances which are formed during that proceſs. This idea, however, can only be explained according to Dr. Mitchell's theory of the formation of the contagium; and the uſe of the oil conſidered in this point of view, can only take place if the principle of the contagium is lodged in the ſkin, without circulating in the blood, as in that caſe the oxydation may more probably proceed in the lungs.

"3. It cauſes an accumulation of heat in the body, but eſpecially in the ſkin, by being not ſo good a conductor of heat as the atmoſpherical air, and likewiſe by diminiſhing perſpiration, which deprives the body of heat.

"4. It produces a *plethora partialis* in the ſkin, by the accumulation of ſerous humours that are generally excerned by perſpiration, whereby the excitement, particularly of the ſkin, increaſes; and if this is heightened to a certain degree by the particular ſtate of the body, or by other circumſtances, a general ſweat ſupervenes, by which the excitement is again diminiſhed, and the equilibrium reſtored. This ſweat is extremely beneficial, as it expels the poiſon out of the body before it produces the diſeaſe; it is generally very copious, and breaks out at once over the whole body, on which account it proves critical, and is preferable to a ſweat excited by any diaphoretic medicine.

"This method of applying the oil deſerves, therefore, to be firſt ranked amongſt the different remedies that have been recommended for the purpoſe of preſerving the body from infection, and poſſeſſes the double advantage of being cheap, and requiring very little time and care, and making the daily change of clothes and the fumigations unneceſſary, on which account it is particularly calculated for the poorer claſs of people. It may, however, be obſerved, that the oily frictions are not able to prevent all poſſibility of infection; for though the moſt

moſt common way in which the contagion is received into the body, is by the ſkin; yet it ſeems very probable, that it may likewiſe penetrate into the body through the lungs, the mouth, and the inteſtinal canal, a circumſtance that is proved by ſeveral obſervations.

" Olive oil is probably not preferable to any other unctuous ſubſtance for the purpoſe of anointing the body; and in countries where it is not to be had ſo eaſily and ſo good, it may be replaced by any other oily ſubſtance. The addition of wax or ſpermaceti will improve it, and prevent its becoming ſo ſoon rancid. The oily covering ought to be waſhed away from time to time. There is no reaſon to fear that the oil may do harm to health, by ſuppreſſing the perſpiration; but though this is diminiſhed by the oily frictions, it ſeems by no means to be wholly ſuppreſſed; and in caſe it ſhould be ſo, that function of the ſkin is eaſily replaced by the lungs, the kidneys, and the inteſtinal canal.

" For explaining the healing powers of oil againſt the plague, when it has already appeared, Dr. Scheel endeavours to prove from the obſervations of different authors, that the plague is more frequently of the ſthenic than of an aſthenic nature. The characteriſtic properties of the *contagium pestis* appear to be the following:

" 1. It is an extremely ſtimulant and penetrant poiſon, of certain chemical and phyſical properties, which are ſtill unknown.

" 2. The diſeaſe which is produced by it, keeps no certain and regular periods, like the Small-pox.

" 3. The poiſon of the plague poſſeſſes a particular tendency of cauſing tumours in the lymphatic and ſalival glands, and in the tela celluloſa of the ſkin, which frequently diſappear again without any ulceration.

" 4. The criſis of this diſeaſe is more rarely perfect than in other contagia. It takes place by a copious ſweat, which removes the pyrexia of the plague, and at the ſame time evacuates the poiſon, or the diſeaſe ceaſes by inſenſible perſpiration, or by the ſuppuration of an eruption attending the plague. Dr. S. is, however, inclined to conſider the buboes merely as a ſymptom, but not as a criſis of the plague, becauſe ſometimes they do not appear at all, and ſometimes at the ſtadium cruditatis, and very frequently the diſeaſe terminates ſucceſsfully without them, merely by a copious ſweat.

" 5. It does not render the perſons who have experienced the plague incapable of being again affected with the contagion, as is the caſe in the Small-pox, for the ſame perſons are again

again expoſed to infection, even during the ſame epidemic, which, however, but rarely happens.

"The infection proceeds in different ways; ſometimes the diſeaſe ſuddenly evolves itſelf by a great quantity of poiſon having entered the body; but if only a ſmall quantity of it is received into the frame, ſome time is required before it can be ſo much augmented as to be able to produce the diſeaſe. It likewiſe depends on the ſubject and his particular diſpoſition, whether the diſeaſe comes on ſooner or later. It has been generally remarked, that it is promoted by a ſthenic diatheſis, as in this caſe the acceſſion of any ſtimulus is likely to produce a hyperſthenia and an indirect debility. The contagion alſo is accumulated in the ſkin by the ſuppreſſion of perſpiration; but it acts more powerfully when it is received through the noſe or with the ſaliva, and in this way immediately communicated to the circulation. There is alſo ſome difference in the phenomena of the diſeaſe depending on the particular nature of the epidemic, which is in ſome inſtances more or leſs malignant.

"It has not yet been aſcertained, whether a particular ſtate of the humour influences the infection, and why the contagion affects particularly the inteſtinal canal in ſome epidemics, and in others the nervous ſyſtem.

"The diſeaſe is generally ſthenic in the beginning, but becomes aſthenic in perſons whoſe food is bad, or in whom fear and grief have produced an aſthenic diatheſis. The plague is always aſthenic in its progreſs, even in robuſt people, from an indirect aſthenia, and in ſome inſtances the poiſon may act ſo violently, as almoſt immediately to produce an indirect debility, becauſe the previous hyperſthenia is of ſo ſhort a duration as hardly to be perceived. The diſeaſe terminates fatally by diarrhœas and hæmorrhagies ſupervening. After having premiſed theſe few obſervations on the nature of the plague, the action of the oil may be eaſily determined, which accordingly appears to conſiſt, 1. In affecting a critical ſweat, that evacuates the poiſon and abates the pyrexia. 2. In diminiſhing, under certain circumſtances, too copious ſweats, and preventing a debility that ariſes from them. The critical ſweat that is occaſioned by the frictions with olive oil, ought to be carefully promoted and ſupported by antimonials, and to which, in caſe of a tendency to diarrhœas, opiates may be very properly added; and when the ſweat begins to weaken the patient, broth, with eggs, wine, &c. ought to be adminiſtered. It has been obſerved, that ſleep is extremely dangerous while the ſweat laſts; for it ſeems as if the ſyſtem required all the natural ſtimuli for rendering this ſweat critical, and the leaſt deficiency of them would produce a contrary effect. It may be ſometimes adviſable to add turpentine or juniper oil, in order to increaſe the ſtimulus in an aſthenic

asthenic diathesis. The vomiting and diarrhœa require the utmost attention in stopping them. The frictions with oil are also very serviceable in other diseases, for the purpose of counteracting contagions, and preventing an immoderate sweat. Mr. Baldwyn relates two instances, where the spots anointed with oil remained free from the eruption in a child that had been inoculated for the Small-pox, &c."

To the Editors of the Medical and Physical Journal.

Gentlemen,

Permit me, through the channel of your valuable Journal, to request a *drawing*, or more minute description, of an instrument for extracting Teeth, invented by Mr. Whitford, and strongly "recommended to the attention of the Medical world," by Mr. Reece, of Craven-street. Being much in the practice of extracting Teeth, I have always considered it a desideratum to meet with an instrument which would take out a tooth, not loosened by disease, with less pain than those in common use.—Many such have been proposed by ingenious men, which have equally failed in the practical application of them. The first invention for raising a tooth perpendicularly, I believe, was published by the late Dr. John Aitken, of Edinburgh, in his "Essays on Important Subjects in Surgery;" but neither this, nor any other similar instrument, has been found to answer the purpose, till the one now invented by Mr. Whitford, which Mr. Reece assures us he has "repeatedly made use of with great satisfaction to himself," and that "his patients have uniformly expressed their surprise at the slight pain they experienced in the operation." I confess Mr. R's great success very much *surprises* me, if the *peculiarity* of this instrument consists, as Mr. R. describes, in "the heel of the instrument being applied to the tooth, exactly opposite the point of the claw." This may be done in the common key instrument, but with the almost certain effect of breaking off the corona of a firm tooth. For it is obvious, that in such an application of the instrument the resistance on one side of the tooth is equal to the force applied to the other; and, consequently, that nineteen teeth out of twenty, if their fangs are firm in the alveolar processes, will inevitably be broken. But by placing the fulcrum or heel of the key instrument considerably lower than the fixed point of the claw, the tooth is made to turn on its axis, and may generally

generally be easily extracted, though the alveoli must be broken. The merit of Mr. Whitford's instrument must then, I humbly conceive, depend upon the peculiar manner in which the action of the fulcrum and lever is performed, and which Mr. Reece will, perhaps, in a future number of your Journal, obligingly describe. To me it appears impossible to *apply* any instrument to the farthest molards, which can raise them in a perpendicular direction; nor can I conceive how any tooth can be thus extracted without the fulcrum resting on a neighbouring tooth, to the manifest risque of injuring it. The theory of raising teeth perpendicularly is pretty, but, I apprehend, can never be reduced to practice by any instrument, excepting when the teeth are loose; for, when they stand firm, such force must be applied to raise them perpendicularly as would greatly endanger whatever the fulcrum rested upon. When a tooth (as often happens) is entirely broken down to the gum on one side, and a bare shell is left on the other, the placing the claw and heel of the instrument opposite to each other would immediately snap it off, without stirring the fangs, provided they stood firm.—That loose stumps and loose whole teeth may be extracted in nearly a perpendicular line, I can easily conceive; it is often practicable with a pair of forceps. The difficulty and objections to attempts of this sort apply principally to the extracting teeth which stand firm in their sockets, or are entirely broken away on one side, and more especially when they are level on both sides with the gum, which frequently happens. I should be happy to be informed by Mr. Reece whether he has found the instrument to answer under all these circumstances, and request he will have the goodness to give the public a more particular description of its mode of action in "elevating the tooth in nearly a perpendicular direction."

I am, &c.

GEO. CUSTANCE.

Kidderminster, Aug. 14, 1801.

CRITICAL RETROSPECT
OF
MEDICAL AND PHYSICAL LITERATURE.

Genius der Gesundheit und des Lebens; i. e. Genius of Health and Life, a Pocket Book for Phyſicians and others, for the Year 1801, by Dr. C. J. Kilian. Leipzig, Wiegel, price 16 groſhen, or about 2s. 6d.

This publication is intended for the uſe of the public at large, in order to divulge better and more enlightened notions on objects of medicine, by which means the author, who occupies an honourable place amongſt the medical writers of Germany, thinks to baniſh ſeveral prejudices that are ſtill reigning amongſt the people, with reſpect to medical aſſiſtance. For this purpoſe he here gives: 1. *Newest theory of general medicine, theoretical as well as practical.* We doubt very much whether this Memoir will anſwer the purpoſe, as it is too philoſophically written to be ſufficiently underſtood by every reader; and it ſeems to us not at all proper to propoſe an entire ſyſtem of medicine in a popular work, as by being frequently miſunderſtood, it is apt to convey wrong and imperfect ideas to the reader. 2. *Fragments of a domestic materia medica.* It is the deſign of the author to give a ſurvey of all the domeſtic remedies, which will be yearly continued. This is indeed a laudable undertaking, as that part of medicine has been hitherto very little attended to. The author treats here of milk and eggs. 3. *Some dietetic observations and precautions for smoakers.* No perſon will wonder at finding ſuch an article, who knows that the cuſtom of ſmoaking tobacco is ſtill prevalent in Germany amongſt the higher claſſes of people; rules, therefore, concerning that habit, are not foreign to this publication, and they will be found entertaining, as the author gives them with much wit and humour. 4. *Two medical observations.* 1. On the effects of bathing in rivers, which, in ſome caſes, is very prejudicious to health. 2. On the uſe of biſhop, (a liquor compoſed of red wine, ſpices, ſugar, &c. like negus) in a caſe of hæmoptoë.

A Treatise on the Cow-pox, fully explaining its Causes, Symptoms, Inoculation, Treatment relative to other Eruptions on the Skin of Men and Beasts, according to Observations made by the Author. By Frederick Benjamin Osiander, M. D Profeſſor of Medicine and Midwifery in Gottingen. With coloured copper-plates. Gottingen, 1801, octavo, pp. 238.

The author is the firſt phyſician who inoculated with the Cow-pox ſucceſsfully in Gottingen, and earneſtly recommended it by powerful arguments in an expreſs publication. The copper plates accompanying it, which are well engraved and carefully coloured, re-

repreſent

represent the hand of a milkmaid having a Cow-pock, and also another plate in folio, representing the course of the daily appearance of the Cow-pox, according to Prof. Osiander's method of inoculating with a blister.

The drawing is done from Nature by Dr. Osiander himself, and the engraving by Basemann, a good painter and engraver in Gottingen. It is a very exact delineation of the Cow-pox pustule on the place of inoculation, in its various changes throughout the whole course of the disorder.

An Essay on the Proximate Cause of Animal Impregnation, being the Substance of a Paper read and discussed in the Medical Society at Guy's Hospital, in October, 1799. By JOHN PULLEY, of Bedford, Member of the Royal College of Surgeons, London. 4to. pp. 31. London, 1801, Cox, &c.

Mr. P. in his Preface says, " It is the sole intention of the author to prove by arguments and facts, the inefficacy of the doctrines taught by Dr. Darwin and Dr. Haighton, on the mysterious subject of animal impregnation;" and not those two physiologists only, but " all who give exclusively to one sex the power of reproduction.

" In tracing the process of generation," says Mr P. " theorists have ranged themselves on three distinct grounds, each of which has had its ardent advocates, and as strenuous opponents. One gives to woman alone the humble office of affording a proper nidus for the due evolution of the fœtus, which according to this theory already exists in the male semen, and requires only a fruitful habitation. Another directly reverses this position; it puts the female in possession of every requisite for the formation of a new animal, and considers the male a mere stimulating engine to call the latent powers of the female into life. The third gives not pre-eminence to either sex, but with the mutual embrace produces a mutual effect; it regards both the male and female as most essentially concurring in the work of reproduction, each affording a something, which, uniting under proper circumstances, becomes the proximate cause of impregnation.

" Although every existing theory on the reproduction of animals is reducible in its principle to one of the above grounds, the warm and fertile imagination of speculative minds has led to almost innumerable modifications, each theorist assuming his fundamental position, and forming his deductions in manner and respect to the direction of his fancy."

After combating the opinion that *corpora lutea* are decisive proofs of impregnation, he says, " What a train of evidence, embracing facts most positive and indisputable, does that author call up against him, who maintains that the male semen alone possesses the power of stimulating the os uteri and adjoining parts, and that by sympathy generation is effected. When a negro man embraces a white woman, why is it that the offspring is a mulatto? When a male ass copulates with a mare, why does the mule partake of the nature

nature of both? And when dogs and bitches of different species have intercourse, why in appearance do the mongrel whelps claim affinity to both parents? Again, it is a self-evident truth that a child may inherit the disposition to the constitutional diseases of either parent; and shall it be said, that it is in the power *of sympathy* to hand down to posterity the contaminated habit of the father? Besides, if the semen be allotted merely to stimulate the uterine system, it would seem a totally unnecessary secretion; for we find that the sexual act is not wanting, even to effect those changes, which the semen, by this theory, is only permitted to perform."

A Treatise on Febrile Diseases, including Intermitting, Remitting, and Continued Fevers; Eruptive Fevers, Inflammations, Hæmorrhagies, and the Profluvia; in which an Attempt is made to present, at one View, whatever, in the present Sate of Medicine, it is requisite for the Physician to know respecting the Symptoms, Causes, and Cure of those Diseases. By A. P. Wilson, M.D. F.R.S. Ed. Fellow of the Royal College of Physicians, Edinburgh, &c. Vol. II. 8vo. pp. 580. Winchester, 1800. London, Cadell and Davies, Callow, &c.

Our readers will see our account of the First Volume in the second vol. of the Med. Journal, p. 300. That volume having exhausted intermittents and continued fevers, Dr. W. postpones the Phlegmasiæ of Dr. Cullen, and in this second volume completes his First Part, of *Idiopathic Fevers*, with the *Exanthemata*.

In the preface to this second volume, Dr. W. replies to some criticisms on the first, and, as we predicted, his observations on the Brunonian system and nosology have not escaped notice.

He replies, "The Brunonian doctrines have been so warmly contested, and so frequently midated, that I examined them with a degree of caution and minuteness, which would not otherwise have been necessary. And aware as I was of the hypothetical manner in which the proximate cause of fever had been treated by every other writer as well as Dr. Brown, it will be admitted, I hope, that I have not departed from a due degree of caution in any of my observations on this subject.

"With all the care I was capable of, however, I have not succeeded in conveying the same ideas to every reader. It has been stated by one, that I espouse the general principles of the Brunonian system; by another, that I admit no part of it but that which was admitted by all physicians, before Dr. Brown's Elements of Medicine appeared. By one, many of my objections to this system are regarded as invalid; by another, their validity is admitted, and I am censured for allowing it any merit at all. I am said by one to aim at extending the Brunonian system; by another, accused of attempting to bring about a coalition between the systems of Dr. Cullen and Dr. Brown, which the critic more justly than elegantly observes, is as hopeless a task as endeavouring

ing to milk he-goats. What ſhall I ſay in anſwer to ſuch contradictory objections? I ſhall only obſerve of the two laſt, that I am perfectly unconſcious of having made either the one attempt or the other. All I have attempted is to give an accurate view of the Brunonian ſyſtem, to ſeparate the true from the falſe parts of it, and to arrange certain facts relating to the laws of excitability without reference to any ſyſtem whatever. I ſhall here endeavour in a few words to place the reſult of what was ſaid of the proximate cauſe of fever in a clearer point of view.

" When a ſtate of exceſſive excitement or atony exiſts independently of the continued application of ſome artificial agent, one of two changes muſt have taken place; either the quantity or quality of the natural agents, or the ſtate of the living ſolid, is different from that which prevails in health. If it can be ſhown that the ſtate of the living ſolid remains the ſame, it follows that the deviation from health is owing to ſome change in the natural agents; if it can be proved that the ſtate of theſe agents remains the ſame, it then follows, that the deviation from health is owing to ſome change in the ſtate of the living ſolid. We may go a ſtep farther; if it can be proved that ſome of the natural agents remain unchanged, and yet produce effects different from thoſe they produce in health, it not only follows that the ſtate of the living ſolid is changed, but alſo that, if this change in the ſtate of the living ſolid will account for the changes obſerved in the effects of other natural agents, we are not in any degree to attribute ſuch effects to a ſuppoſed change in theſe agents, there being no occaſion for any ſuch hypotheſis to explain the phenomena. In fever, many of the natural agents, caloric, food, light, noiſe, for example, evidently remain unchanged, the difference in their effects, therefore, is owing to a change in the ſtate of the living ſolid. But this change is capable of accounting for the change we obſerve in the effects of thoſe agents whoſe condition we cannot with preciſion aſcertain, the circulating and other fluids. It follows, therefore, that whatever change may take place in theſe during the progreſs of fever, and however this change may modify the ſymptoms of fever, too great lentor, acrimony, or other morbid condition of the fluids, is not the proximate cauſe of fever.

" With reſpect to the hypotheſis of fever depending on a change in the ſtate of the ſimple ſolid. As the natural agents act not on the ſimple but on the living ſolid, it is neceſſary to ſuppoſe a change in the ſtate of the latter; and as this change accounts for the phenomena of fever, there is no occaſion for any other ſuppoſition.

" And farther, as all the natural agents excite a morbid action, and as this effect is not confined to any one, but obſerved equally in every part of the ſyſtem, what room is there for ſuppoſing that any one part is more particularly affected than every other?

" Laſtly, with regard to fever being a ſtate of accumulated or exhauſted excitability, in the ſenſe in which Dr. Brown uſes theſe

terms,

terms, it is only neceſſary to refer to the facts, which prove that no ſuch morbid ſtates exiſt. It is true that the phenomena of ſynocha are ſuch as we ſhould expect from an accumulation of excitability; but will a ſurfeit, or an exceſſive quantity of diſtilled ſpirits, frequent cauſes of ſynocha, occaſion an accumulation of excitability? It appears then, that in fever the living ſolid is ſo changed, that a change is effected in the laws of its excitability, and that this admitted, there is no occaſion for any of the foregoing hypotheſes to explain the phenomena eſſential to fever. Upon the whole then, the following, as far as it goes, would appear to be a juſt view of the nature of fever.

"Every agent acting on the ſyſtem in general is capable of producing three effects, moderate excitement, exceſſive excitement, or atony, according to the degree in which it is applied The firſt operation of agents conſtitutes health; the two laſt general diſeaſe, which has been called fever. If, by the application of artificial, or the exceſſive application of natural agents, either of the two laſt ſtates be maintained for a ſufficient length of time, the living ſolid is ſo changed, that is, ſuch a habit is formed, that the natural agents applied in the uſual degree produce the ſame morbid effects till the diſeaſed habit has been counteracted; which, as in the caſe of other habits, is the more eaſily effected, the ſhorter its duration has been. Hence it is that almoſt any thing making a ſtrong impreſſion will ſometimes remove fever at an early period; and hence the difficulty of removing a fever is generally proportioned to the time it has laſted. The means which cure a fever at an early period, that is, produce a criſis, ſeem either to expel the offending cauſe before the morbid habit is effected, as vomiting during a fit of drunkenneſs; or break the morbid habit before it has gained force, as cold-bathing during the firſt days of fever. In a more advanced ſtage, as the morbid habit is corrected with more difficulty, it is corrected more ſlowly. When in ſynocha we ſucceed in changing exceſſive into moderate excitement, h. e. into that excitement which is followed by exhauſtion, we have removed the morbid habit, and conſequently cured the fever. The cure of ſynocha, therefore, depends on the abſtraction of ſtimuli. But as atony is the conſequence of exceſſive excitement, if exceſſive excitement has laſted for a conſiderable length of time, atony will always be evident, previous to the reſtoration of health. Hence it is, that the ſymptoms of typhus ſucceed thoſe of ſynocha. When we ſucceed in changing atony into moderate excitement, we have corrected the morbid habit, and conſequently cured the fever. The cure of typhus, therefore, depends on the addition of ſtimuli.

"I have been indirectly accuſed of having aſcribed too much importance to the ſtudy of noſology. It is faſhionable at preſent to regard noſology as a very uſeleſs branch of medicine. Will the knowledge of a noſological ſyſtem, it is ſaid, enable you to cure a diſeaſe? It certainly will not, but it greatly aſſiſts in acquiring the knowledge that will. Let thoſe who ſlight the labours

bours of the nosologist, recollect that it is his province to point out the symptoms which distinguish one disease from another, and to arrange diseases in such a way as may best shew their affinity, and consequently assist the memory in recollecting their modes of treatment. The anatomist detects the changes induced by internal disease; but of what use would this knowledge prove, did not the nosologist point out the means of ascertaining the presence of such morbid states previous to death. In vain might the chemist and botanist supply us with medicines, did not the nosologist enable us to distinguish the cases in which they are useful.

"It has been asserted, that the practice of medicine would be improved by attending to symptoms individually, without attempting to ascertain their various combinations, and applying to these combinations particular names. Those who make this assertion maintain, and if the assertion is true, justly maintain, that nosology is an useless study. Is the assertion true? Does any symptom at all times require the same mode of treatment? Nay, is not the same symptom in one case salutary, in another pernicious? We must, therefore, be influenced in treating each symptom by an attention to those which accompany it, that is, an attention to the combinations of symptoms is necessary, and consequently nosology of the first importance. By far the greater number of mistakes I have witnessed in practice, have originated from the neglect of nosology. It often happens that an opinion, at first maintained on no other account than its singularity, becomes current among those who are unable, or will not be at the trouble, to think for themselves. Many exclaim against nosology, but cannot tell why. The truth is, an accurate knowledge of it is acquired with difficulty, and the indolent are glad of an apology for neglecting it altogether."

He concludes his preface thus:

"Contrary to my first intention, the present volume includes all the species of eruptive fevers, and consequently finishes the first part of the work, comprehending idiopathic fevers. The two volumes now published, therefore, form a Treatise on Idiopathic Fevers, and may be regarded as independent of those which are to follow.

"The Symptomatic Fevers will form the three remaining volumes. In the first of which, that is, the third volume of the work, the inflammations of the skin, head, and neck, will be considered; in the fourth, those of the thoracic and abdominal viscera, and of the joints. The last volume will comprehend the hæmorrhagies and profluvia, with a more detailed view of the nosology of febrile diseases.

"It is my intention, when the present work is finished, to commence another, (which will consist of two volumes, and for which the materials are already collected) on the Nervous Complaints most frequently complicated with Febrile Diseases."

In the volume before us, the author, in concluding his second book, treats of the *varieties* of continued fever, viz. petechial, miliary, aphthous, vesicular, and erysipelatous.

The

The third book contains the Exanthemata, viz. variola, vacciola, varicella, rubeola, fcarlatina, peftis, and urticaria.

As our readers have probably more curiofity refpecting the Plague than either of the other difeafes treated of in this volume, we fhall prefent them with a few extracts on that fubject:

"*Of the Plague.*

"Although few Britifh phyficians have occafion to practice in the plague, the propriety of being acquainted with a difeafe, which has demanded fo much attention, and bears fo ftrong an analogy to complaints which every day fall under their care, is too apparent to require any comment. Befides, we cannot forefee in what circumftances we may be placed; and for a phyfician to betray ignorance of the plague would be unpardonable.

"There are few difeafes fo remarkably varied, and there are few, perhaps none, of which it is more difficult to give an account, which fhall be at the fame time fufficiently full and diftinct. The reader will find the beft writers, who have had an opportunity of feeing the difeafe, complaining of this difficulty.

"As much as poffible to prevent confufion, they have divided the plague into different claffes; nor is it poffible without this to give a juft view of the complaint, fince no two difeafes are more oppofite than the different forms of the plague. In one we fhall find it the moft dreadful of all fevers, deftroying without exception all whom it attacks; in another we fhall find it confifting chiefly of an eruption unattended by danger. Why, it may be faid, are difeafes fo different, regarded only as varieties of the fame? However different thefe extremes, they are not only produced by the fame fpecific contagion, but almoft infenfibly run into each other; from which we may form fome idea of the variety which the plague prefents. It will not then appear furprifing that the difficulty of arranging its fymptoms, fo as on the one hand to avoid confufion, and on the other to give a comprehenfive view of the complaint, be very great. In fact, there is no author, although moft of thofe who have written on the plague fpeak from their own obfervation, who has overcome this difficulty. In all, we find the account either fhort, and confequently more or lefs imperfect, or of confiderable length, and more or lefs confufed. So varied are the fyptoms of the plague, that if the common varieties are given, the account muft be tedious, and then it is impoffible perhaps to prevent it being in fome degree perplexed.

"I fhall not fpend time by laying before the reader the modes of divifion adopted by different writers, or by pointing out the objections which might be made to them. The objections confift chiefly, in many of the divifions not being marked with fufficient precifion, fo that it is often impoffible to fay what are the correfponding divifions in the different accounts of the difeafe; as the reader will perceive if, for example, he compare together the different accounts of the plague in the Traité de la Pefte, or any of

these with the division adopted by Dr. Russell, in his Treatise on the disease.

" In dividing a disease into varieties, each variety must be marked by some symptom which constantly attends it. It must not be accidental or unconnected with the state of the symptoms in general, which would render the division useless; but must mark a variety, in which the symptoms on the whole, and, what renders the division of more importance, the prognosis, differ from those of other forms of the complaint.

" I shall not defer a particular account of the eruptions, namely, the buboes, carbuncles, &c. till after the different forms of the disease have been considered, as has usually been done; by which we are forced to use terms before they have been defined; nor on the other hand, is it proper, where the variety is so great, to interrupt the account of the general course of the disease, in order to describe the different eruptions. It therefore appears necessary to depart from the order which has been pursued in laying down the symptoms of the other exanthemata, and to regard bubo, carbuncle, &c. as terms which must be defined, before proceeding to give the symptoms of the plague.

" *Of Pestilential Eruptions.*

" 1. Of PESTILENTIAL BUBOES.

" A pestilential bubo at its commencement is a small, hard, round tumour, readily perceptible to the touch, about the size and shape of a pea, it is moveable under the skin, the appearance of which is not altered at an early period, the bubo lying at a greater or less depth, and the swelling not appearing externally.

" As the tumefied gland enlarges, it changes from a round to an oval shape, becoming at the same time less moveable. The integuments now begin to thicken, and the swelling to appear externally.

" The appearance of the bubo is often preceded by a sense of tightness and pain sometimes lancinating, or itchiness, in the part where it is about to appear, now and then by shivering. In many cases, however, the small swelling just described comes on without being preceded by any peculiar symptoms.

" Some buboes are indolent and insensible, others very sensible and rapid in their progress. The tumour advancing quickly to suppuration, is generally regarded as favorable. When the buboes suppurate properly, De Mertens observes, and there is a separation of eschars from the carbuncles, with a remission of the febrile symptoms, the prognosis is good. No general rule however can be laid down. Cases where early suppuration takes place often prove fatal; and there are many histories of cases terminating favorably where the buboes were extremely indolent and terminated in resolution.

" It is difficult to foresee in what way a bubo will terminate. The fluctuation is often scarcely perceptible where suppuration has taken place, and buboes are sometimes resolved after fluctuation has

has been very evident, Their progreſs indeed is almoſt always more or leſs irregular, eſpecially after the firſt week. At one time they ſeem advancing to ſuppuration, at another ſhow a tendency to reſolution. 'But theſe variations,' Dr. Ruſſell remarks, 'chiefly 'reſpected the integuments; for the gland itſelf when carefully 'explored was ſeldom found to alter, and where the tumour actually diſperſed, it was not ſuddenly, but by ſlow degrees. Thus 'from the alteration in the teguments alone, the whole tumour, on 'a ſuperficial view, ſeemed to leſſen or increaſe, though the gland 'remained the ſame; and I am inclined to think that this deception 'was often the cauſe of the bubo being ſaid to fluctuate, or to 'vaniſh in appearance entirely, and again return.' He adds, however, 'At the ſame time I am far from thinking that this fluctuation was never real.' And Chenot obſerves, 'Vidimus quoque 'abruptam ſuppurationem in his reſuſcitari ac demum per effuſionem puris abſolvi.'

"The bubo as it increaſes in ſize becomes ſomewhat flat; and generally about the ſecond week the ſkin over it grows tenſe and painful, and begins to be inflamed. In ſome caſes the inflammation is moderate, in others conſiderable; but it ſeldom terminates in gangrene, although the ſkin now and then aſſumes a bluiſh colour.

"It ſometimes happens, however, that the bubo runs to ſuppuration without any degree of inflammation appearing on the ſkin, and then, as it is generally harder than a ſuppurated venereal bubo, it is often difficult to determine whether ſuppuration has taken place or not. When buboes break ſpontaneouſly it generally happens in the third week, ſometimes at a later period.

"The buboes moſt frequently appear in the groins or a little lower, among the loweſt cluſter of the inguinal glands; they alſo frequently appear among the axillary glands; ſometimes, though more rarely, they have their ſeat in the parotid, and the diſeaſe is then by many reckoned more dangerous than when the buboes appear in the groins or arm-pits. Still more rarely they appear in the maxillary or cervical glands.

'The latter too,' namely, the maxillary and cervical glands, Dr. Ruſſell remarks, 'were ſeldom obſerved to ſwell without either the 'parotid ſwelling at the ſame time or ſoon after, or a carbuncle protruding near them; they never were the ſole peſtilential eruptions, 'and I recollect few inſtances of their coming to maturation.' It has been remarked by others, that the parotid bubo ſeldom appears unaccompanied by one or more in the axilla or groin.

"It may upon the whole be obſerved, that the axillary buboes ſuppurate more frequently than thoſe ſituated about the fauces, and the inguinal more frequently than the axillary.

"Buboes often make their appearance on the firſt day of the complaint; ſometimes indeed they are among the firſt ſymptoms. It has been obſerved, that when they appear later than the third or fourth day, they are generally preceded by an exacerbation of the febrile ſymptoms. Thoſe which come out at ſo late a period, however,

however, are not, for the moſt part, the firſt which appear in the courſe of the complaint; for a ſucceſſion of buboes ſometimes takes place, till three or four have made their appearance. In this caſe ſeveral hours uſually intervene between the appearance of any two of them.

"It ſometimes happens that no buboes appear, and theſe caſes are upon the whole the moſt fatal. This is a circumſtance which particularly demands attention, as the caſes unattended by buboes and other peſtilential eruptions generally make their appearance at the commencement of the epidemic, and have often, in conſequence of the abſence of the eruptions, been miſtaken for other complaints. In other caſes, particularly towards the decline of the epidemic, the buboes and other eruptions often form the principal part of the complaint, which is then unattended by danger; from which it would appear, that the eruptions in the plague are to be regarded as favorable ſymptoms; but of this I ſhall preſently have occaſion to ſpeak more particularly.

"Where the inflamed gland advances to ſuppuration more rapidly than the integuments, troubleſome fiſtulous ulcers are ſometimes formed, if an artificial opening has not been made in the ſkin. This accident however is rare; in general the buboes, left to themſelves, do not prove troubleſome.

"When they do not ſuppurate, and the patient recovers, they gradually diſperſe, generally in the ſpace of a few weeks. In ſome caſes they are ſucceeded by an induration of the gland, which remains for many months. Even where ſuppuration has taken place, if the cure proves tedious, either in conſequence of the matter having been diſcharged by too ſmall an opening, or the opening having repeatedly cloſed in the progreſs of the cure, a ſimilar induration ſometimes ſucceeds, which in like manner ſooner or later diſappears, theſe indurations never terminating in cancer.

"Such are the circumſtances to be learned from attending to the external appearances of the buboes; ſome further circumſtances, of leſs moment however, have been aſcertained by diſſection.

"It has been the practice of many, particularly the French ſurgeons, to extirpate the buboes, which gave them an opportunity of obſerving the internal changes which take place in them.— From the appearances on diſſection they have been divided into ſeveral different ſpecies. It is unneceſſary to detain the reader with an account of this hitherto uſeleſs diviſion; he will find it at length in the Traité de la Peſte from the 428th to the 434th page. One obſervation deſerves attention; it has juſt been remarked, that the ſkin covering the buboes never runs to gangrene; diſſection ſhows that it is otherwiſe with reſpect to the gland itſelf. 'Je coupai par le milieu celle (h. e. the bubo) qui etoit ſur les 'vaiſſeaux, que je trouvai toute noire. Le lendemain j'ouvris le 'bubon, j'y trouvai le corps glanduleux comme un rein de mouton, 'tout noir.'

"Beſides the true bubo, another peſtilential eruption has alſo received the name of bubo. This eruption is ſo rare that ſome who

who mention it have been accuſed of miſrepreſentation. This accuſation we are now aſſured is groundleſs.

"The principal circumſtance in which the ſpurious differs from the true bubo, is in the former appearing indiſcriminately on almoſt every part of the body, while the true bubo is confined to the groin, axilla, and parts about the fauces. 'Spurious buboes 'were obſerved,' ſays Dr. Ruſſell, 'on the head, the forehead, 'the throat, the ſhoulder, above the clavicle, the neck, on or 'above the ſcapulæ, the back, the ſide under the breaſt, the belly, 'the hip, hind part of the thigh near the ham, the leg, the 'ſcrotum, the arm near the uſual place of iſſues, inſide of the arm 'near the elbow, outſide of the fore arm, and near the wriſt.'

"Some of theſe buboes, if they are not lanced at a proper time, grow to a great ſize, particularly thoſe on the ſcapulæ or back; in other parts, however, they ſeldom much exceed the ſize of a common hen's egg. They generally make their appearance about the ſecond or third day, and for the moſt part after the protruſion of true buboes or carbuncles. They generally ſuppurate, though leſs rapidly than the true buboes.

"2. Of Carbuncles.

"Next to buboes, carbuncles are the moſt remarkable of the peſtilential eruptions.

"The reader will find carbuncles divided by different writers into ſeveral varieties. One makes three, another four, a third five different kinds.

"Dr. Ruſſell divides the carbuncles he met with, into five varieties.

"The firſt appeared in the form of a ſmall puſtule about the ſize of half a pea, on its upper ſurface of a duſky or yellow colour, and a little wrinkled. The ſkin which immediately ſurrounded this puſtule was hard and inflamed. The puſtule itſelf ſoon became very painful, and continued to increaſe till it became a tumour of the ſize of a nutmeg, and ſometimes that of a walnut, and a yellowiſh matter was ſecreted under the cuticle, which was ſometimes moiſt, at other times dry and cruſty; the reſt of the tumour aſſumed a dark reddiſh colour, the circle which ſurrounded it appearing at different times of various hues.

"On the third, fourth, or fifth day of the carbuncle, a gangrenous cruſt appeared on the middle of it, which ſoon occupied the whole ſurface of the tumour, exactly reſembling the black eſchar formed by cauſtic.

"This cruſt, when the termination was favorable, was thrown off by ſuppuration, leaving an ulcer of various depth, which for ſome time continued to diſcharge matter. When the caſe terminated fatally, the cruſt remained dry and often ſpread to the inflamed circle, ſurrounding the carbuncle, ſo as to form a gangrene of conſiderable extent.

"The ſecond kind of carbuncle appeared in the form of a ſmall angry puſtule, not riſing ſo high as the former, more diſpoſed to ſpread, and becoming gangrenous on the ſecond day. In this ſtate it

it was not eafily diftinguifhed from the other, but was generally furrounded with a more highly inflamed ring. It chiefly attacked tendinous parts, particularly the joints of the fingers and toes.

" In the third variety, the cuticle was at once raifed into a blifter of the fize of a horfe bean, filled with a dufky yellow or blackifh fluid, and the fkin which furrounded this variety of the carbuncle was lefs tenfe and of a paler red than that furrounding either of the foregoing. When the blifter broke, the cuticle fell upon the flat furface, which was of a dark colour and foon became black. At this period, that is, about the third or fourth day of the carbuncle, it refembled the preceding varieties, except that it was flatter. The circle furrounding the efchar gradually affumed a very dark red, but never became gangrenous. The efchar was about the fize of a fix-pence. This carbuncle was very painful, and five or fix fometimes appeared on the fame patient.

" The fourth variety was a fmall red fpot raifed only to the touch, which gradually rofe higher and fpread, till in twenty-four hours it was a flattifh dufky puftule, furrounded by a light rofe-coloured margin. This carbuncle was very painful, and when it appeared on the face occafioned fwelling, but without inflammation of the fkin. It often became black beyond the rofe-coloured margin on the fecond day, and the mortification fpread to the neighbouring parts. This fpecies of carbuncle always accompanied other eruptions, and was ufually pretty numerous.

" The fifth and laft variety appeared at firft a puftule, which, on the fecond day, refembled that of the fmall-pox; it rofe in the form of a cone to twice the fize of a large diftinct pock with a blunt yellowifh point, which, inftead of advancing to fuppuration, became black to the fize of a large field pea. The gangrene in this cafe however did not fpread farther. The margin became of a dufky red, but appeared brighter as the fuppuration which threw off the efchar advanced. After the fecond day, this differed from the third and fourth varieties only in the gangrenous part being of lefs extent, and the puftule more raifed.

" There are certain eruptions which now and then appear in the plague in fome refpects differing confiderably from any of the carbuncles juft defcribed; in others refembling them. Such is the eruption which has been termed papulæ ardentes, or fire bladders.

" But it would be tedious to enumerate all the various eruptions of this kind which have been obferved in different epidemics.— The true peftilential carbuncle may be defined, a puftular or veficular eruption, fooner or later running to gangrene.

" The eruption called anthrax, is nothing more than a carbuncle after it has become fphacelated.

" Carbuncles, to whatever variety they belong, for the moft part do not exceed the fize of a walnut; they have fometimes been obferved confiderably larger. The time of appearance is uncertain; they fometimes fhew themfelves on the firft day of the complaint, but more commonly not till a later period; and when feveral ap-

pear

pear on the ſame perſon, they generally ſucceed each other rapidly. They have been known to come out as late as the eighteenth or twentieth day.

" Reſpecting the number which appears on the ſame patient, Dr. Ruſſell obſerves, ' Of thoſe of the firſt and ſecond ſpecies ſel-' dom more than one or two were obſerved in the ſame ſubject, in ' general one only. The other varieties occurred in greater num-' ber, and including thoſe of the fifth, I have ſometimes counted ' between twenty and thirty, but this happens very rarely.'

" This eruption is always attended with conſiderable pain, which in ſome caſes is very violent. No external part of the body is exempted from carbuncles. ' I have obſerved them every where,' the author juſt quoted remarks, ' the penis and ſcrotum not ex-' cepted, but never obſerved them on the tongue, the tonſils, and ' internal parts of the mouth,' (there have been inſtances however of their appearing on the tongue) though in carbuncles on ' the ' cheek, near the corner of the mouth, the gangrene ſpreads in-' wards, and in one inſtance of a carbuncle on the eye-brow, the ' gangrene ſpreading upon the globe of the eye had deſtroyed part ' of it.'

" The carbuncle is a leſs favorable eruption than the bubo. Carbuncles were regarded by the Ruſſian phyſicians, Dr. Guthrie informs us, as a ſign of greater malignity than buboes; but of this preſently. They thought the carbuncle indicated leſs danger when red than when livid; when it ſuppurated than when it did not. When the hands and feet were the ſeat of carbuncles, Dr Guthrie obſerves, the patient ſeldom or never recovered. Carbuncles on the ſpine were alſo regarded as particularly unfavorable."

" *Of the other Symptoms of the Plague.*

" That I may abridge the following account of the ſymptoms of the plague, and conſequently render it more diſtinct, it is proper to obſerve, that all the ſymptoms both of ſynocha and typhus occaſionally attend this fever. It is unneceſſary again to detail all of theſe, for which I refer the reader to the firſt volume; and ſhall here conſider at length, thoſe which characteriſe the plague, or appear in this diſeaſe under peculiar modifications.

" It has already been remarked, that in the ſeveral diviſions of the plague adopted by writers, many of the varieties are ill defined. They ſeem to be marked by accidental ſymptoms, and ſome of them by no particular ſymptom, but by the general mildneſs or ſeverity of the diſeaſe. In the former caſe the diviſion can be of no uſe; in the latter it can admit of no preciſion. Beſides, moſt writers on the plague, ſpeaking from their own obſervation alone, deſcribe the complaint as it appeared in one or two epidemics, and in almoſt all epidemics there are peculiarities. It is only by comparing many, that we can form an account of the diſeaſe generally applicable.

" On comparing different epidemics, we ſhall find, that whatever be true of other eruptions, buboes are ſalutary. They almoſt

always mark a form of the disease less generally fatal than that unattended by this eruption. 'Those perished,' Dr. Russell remarks, 'sometimes within the twenty-four hours, sometimes on 'the second or third day; they had neither buboes nor carbuncles, 'and it was very rare to find suspicious marks of infection on the 'dead bodies.' In another place he observes, 'the total absence of buboes in those who died suddenly I have doubt of.' He also remarks, 'that the plague, under a form of all others the most 'destructive, exists without its characteristic eruptions or other ex'ternal mark reckoned pestilential, can admit of no doubt.' The Russian physicians, Dr. Guthrie informs us, found the cases attended with buboes less fatal than those attended with carbuncles. Carbuncles and petechiæ, De Mertens, in his account of the Plague of Moscow, observes, are not critical eruptions, they only denote a putrid condition of the humours; whence it follows, that in proportion as buboes are more common, and petechiæ and carbuncles more rare, the milder is the plague. Orrœus, in his Treatise on the same Plague, observes indeed, that buboes often attended the most acute form of the disease; yet in another place he informs us, that there were no buboes in the worst form, their germs only being sometimes observed after death; and Samoilowitz, in his account of this epidemic, in describing the worst form of the disease, notices petechiæ and carbuncles as frequent symptoms, but makes no mention of buboes.

"Upon the whole, then, the plague unattended by buboes, runs its course more rapidly, and is more generally fatal, than when accompanied by this eruption. The plague may therefore be divided into that which is, and that which is not, attended with buboes.

"The first of these includes many varieties, from that in which the prognosis is almost uniformly good to a form of the disease little less fatal than that unattended by buboes. The appearance of the buboes also affords the means of subdivision, for it will be found, on comparing the accounts of different epidemics, that upon the whole the earlier the buboes appear, the milder is the disease; thus, for example, in the first class of Dr. Russell's division, which was the most fatal, buboes were very rare; in the second, which was also very fatal, though less uniformly so, buboes appeared on the third day or later; in the third class, which was less fatal, they appeared earlier; in the fourth, which was still milder, buboes generally appeared on the first day; in the fifth, which never proved fatal, they were among the first symptoms of the complaint.

"To the two foregoing forms of the disease a third might be added, since the pestilential eruptions towards the end of the epidemic sometimes appear unattended by fever. This form, however, which is merely a local affection unattended by danger, demands little attention.

"The most fatal form of the plague makes it attack in various ways, sometimes merely with depression of strength, a sense of weight

weight in the head, confuſion of thought, giddineſs, dejection, and oppreſſion about the præcordia, often accompanied with a bitter taſte in the mouth.* The patient is inclined to be ſilent, ſhews much anxiety in his countenance, but makes few complaints; the febrile ſymptoms are very moderate. The attendants ſuppoſe the patient a little indiſpoſed, but ſuſpect nothing alarming; yet ſuch patients often die within the firſt twenty-four hours, ſometimes on the ſecond day.

"In general, however, this form of the plague makes its attack leſs deceitfully. In an epidemic deſcribed by Chenot, that of Marſeilles, and many others, the ſymptoms from the firſt were alarming, the complaint often appearing with violent and irregular ſhaking.

"Delirium is ſometimes the firſt ſymptom obſerved. At other times, a remarkable ſtate of the pulſe, which very ſuddenly becomes ſo weak that it can hardly be felt, frequent and intermitting, with much debility and languor, introduces the diſeaſe. The proſtration of ſtrength is ſometimes ſo ſudden and complete, that Mr. Smith, Dr. Guthrie informs us, ſaw men in apparent good health, on being infected by the plague, ſuddenly drop down as if ſhot by a muſket ball. Sometimes, inſtead of mere debility and laſſitude, the patient is affected with extreme horror and deſpair, and his ſpirits ſink ſo low that nothing can recall them.

"At other times, the diſeaſe attacks with very ſlight chills, ſoon followed by a burning heat, which remains during the diſeaſe; as ſoon as the heat commences, the patient complains of inſufferable head-ach and exceſſive thirſt. Sometimes, as in the plague of Ruſſia, deſcribed by Orrœus, the patient is ſuddenly ſeized with violent ſhivering ſucceeded by a hot fit, the ſhivering and hot fit alternating ſeveral times.

"The firſt ſymptom of the plague is ſometimes a violent beating of the temporal arteries, while the pulſe at the wriſt is ſmall and feeble. In this caſe the heat is generally moderate, but the head-ach intolerable. In the plague which raged at Lyons in 1628, a burning heat in ſome of the viſcera, and a dull pain, or rather great heavineſs of the head, announced its approach.

"The plague ſometimes comes on with violent palpitation, and ſtrong convulſive tremblings. The plague which raged in London in 1665, often made its attack in this way.

"As the complaint advances, it aſſumes more of the appearance of the fevers we have been conſidering. The inflammatory ſymptoms generally run high for the firſt day or two; but for the moſt part, the plague aſſumes the form of typhus at

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* The bitter taſte in the mouth the reader will find mentioned by different writers as characteriſtic of the plague. It is obſerved by ſome, that a favourable change ſeldom happened while this ſymptom continued.

an early period; and the patient ſoon becomes delirious or comatoſe.

"The delirium is ſometimes of the furious kind, particularly, Orrœus obſerves, in thoſe of a robuſt and full habit, and in whom a full meal appeared to be the immediate exciting cauſe. In general, however, the delirium is of that ſpecies which characteriſes typhus, the patient appearing rather ſtupid than outrageous, and complaining of a pain at the heart, a ſymptom frequently obſerved in the plague. When coma comes on early, it has been looked upon as affording a worſe prognoſis than delirium, particularly if it ſuffers no evident remiſſions during the day time. Both delirium and coma indeed are almoſt always moſt conſiderable during the night. The remiſſion in the day time is generally more evident when the patient is delirious than when he is comatoſe.

"Whether he becomes comatoſe or not, there is always preſent a very remarkable muddy appearance of the eyes, which is ſometimes obſervable at the very commencement, and is one of the moſt characteriſtic ſymptoms of the diſeaſe. This appearance of the eyes in ſome degree reſembles that in the laſt ſtage of malignant fevers. It is not, however, deſcribed as altogether ſuch, for with the muddineſs there is blended a degree of luſtre. It is an appearance in ſhort very remarkable to thoſe who have ſeen it, but not eaſily conveyed in words.

"Almoſt all writers on the plague take notice of a peculiar caſt of countenance, which to thoſe who are converſant with the diſeaſe, is one of its beſt diagnoſtics. It was the ſtate of the eyes, Dr. Ruſſell remarks, which contributed chiefly to occaſion that confuſion of countenance which he does not attempt to deſcribe, but from which, after repeatedly obſerving it, he could with ſome certainty pronounce whether the diſeaſe was the plague or not.

"The danger is very generally proportioned to the degree of this ſymptom. When the eyes reſume the natural appearance, particularly when this happens after ſweats, the prognoſis is favorable. But in the form of the plague we are conſidering, this hardly ever happens. In the comatoſe the muddineſs of the eyes is moſt remarkable. Their fierceneſs is moſt ſtriking in thoſe who labour under delirium, particularly the furious delirium; and it ſometimes happens, that the coma and delirium, with the peculiar caſts of countenance which accompany them, alternate with each other. In the delirious, however, there is ſtill an appearance of muddineſs, and the eyes retain ſome luſtre in the comatoſe. Theſe appearances of the eyes are leſs remarkable in children than in adults. Such is the beſt account I have been able to collect from the obſervations of thoſe who were converſant with the diſeaſe, of that peculiar appearance of the eyes, which all who have ſeen the plague agree, ſo remarkably characteriſes it.

"The changes which take place in the eye are not always confined to its appearance only; the retina is ſometimes much affected. The patients

patients complain of feeing fparks, flafhes of fire, and various colours paffing before the eyes; this is only a greater degree of the fymptoms termed mufcæ volitantes. Deceptions of fight, however, are not frequent fymptoms in the plague. Deceptions of hearing are ftill more rare. Deafnefs, as in other fevers, is generally a favourable fymptom, but it feldom attends the plague. With refpect to the other fenfes, nothing particular is to be obferved. The depravation of the tafte, which is in fome meafure characteriftic of the plague, I have already had occafion to notice.

" The anxiety in many cafes is extreme, the patient conftantly changing his pofture, and foon finding the prefent as uneafy as the laft, fo that he is fometimes perpetually in motion. When this fymptom is confiderable, it affords a very unfavorable prognofis. The appearance it affumes when at its height, which generally indicates the approach of death, is defcribed by authors, who have termed it a mortal inquietude, in very ftrong terms. The patient inceffantly twifts his body as if in agony, but is incapable of giving any account of his feelings, fo that it is difficult to determine whether it is occafioned by a great degree of anxiety or fevere pain.

" The temperature, in the progrefs of the difeafe, is various. While the chills continue to recur, its increafe is not confiderable, and in the cafes where it is moft confiderable, it feldom equals that which we often meet with in common fynocha.

" The ftate of the pulfe is alfo various. In moft cafes after the firft days of the difeafe, in many after the firft hours, and in fome from the commencement, it is feeble and frequent. Sometimes it is remarkably hard and fmall, but regular, at other times it is irregular or intermitting, and at length fluttering.

" During the exacerbations, it often becomes full, open, and ftrong, as Dr. Ruffell expreffes it; after which it again finks; but in a more advanced ftage, a different change is obferved during the exacerbations, the pulfe becoming fo feeble that it can with difficulty be felt.

" It has been obferved of the pulfe in the plague, that though to a flight touch it is ftrong and full, it is often eafily compreffed; a ftate of the pulfe not readily accounted for, which is mentioned however by more than one author from their own obfervation. The moft ftriking fact relating to the ftate of the pulfe in the plague is, that it has often been obferved nearly natural while the other fymptoms indicated much danger.

" As the difeafe advances, the increafe of debility is generally indicated by a confiderable affection of the fpeech; in fome cafes amounting only to a degree of confufion and faltering, or a change of tone; in others the voice is greatly impaired or wholly loft. The affection of the voice appears the earlier, the greater the debility. When this is exceffive from the beginning, when, as Chenot obferves, 'Ægri erecti ftare aut federe impotes, proprio pondere labebantur,' a confiderable affection of the fpeech is generally obferved

observed on the first night, or the second at farthest. When the debility is less considerable, it is delayed to the third day or later.

" The state of the tongue, as in other fevers, is various. It often retains the natural appearance throughout the greater part of the disease. Sometimes it is moist, and covered with thick mucus, at other times dry. ' Sometimes,' Dr. Russell observes, ' it became parched with a yellow streak on each side, and reddish in the ' middle, but it never was observed to form so thick a fur or become ' of so dark a colour as in the advanced stages of some other fevers. ' The dryness or moistness of the tongue,' he adds, ' rarely corresponded with the febrile symptoms, for the tongue was often ' moist where the external heat was intense, and the pulse indicated ' high fever; and on the contrary, parched where the fever in appearance was very inconsiderable.'

" Vomiting, though more frequently observed in less violent forms of the plague, sometimes attends this variety. The pain at the heart, so frequently complained of, Dr Russell thinks situated about the orifice of the stomach. As it often accompanies vomiting, he was at first led to believe, that it arose from bile or other irritating matter in the stomach. He found, however, that it was not relieved by the discharge. It seems more than probable, from the nature of the symptoms, that this pain proceeds from an inflammatory affection of the stomach. The matter evacuated by vomiting is generally bilious. It is sometimes of a dark colour and mixed with blood; and it is not uncommon in the plague for worms to be thrown out by vomiting. Whatever be the appearance of the matter rejected, when vomiting occurs at an early period and returns at intervals, the prognosis is bad.

" Nausea without vomiting is a frequent symptom. It does not seem to proceed from irritating matter in the stomach, as repeated vomiting is not found to relieve it. There are few means more effectual for allaying nausea and vomiting than those which promote the perspiration. It has been observed of the plague, that if the repeated reaching occasions a moisture on the skin, the nausea abates.

" A diarrhœa is apt to supervene, sometimes during the first days, more frequently at a later period. This is invariably a dangerous symptom. The matter passed by stool is similar to that rejected by vomiting, often bilious, frequently with an evident admixture of blood; sometimes blood only is passed. Chenot often met with dysenteric purging in the plague.

" As there are few fevers in which purging is so unfavorable, so there are none in which constipation appears to be less injurious. ' A number of the sick,' Dr. Russell observes, ' were disposed to be ' costive throughout the disease, and some had no stool for several ' days, the popular dread of provoking a diarrhœa proving a bar ' to laxatives and even to simple clysters, which are readily admitted at other times. The consequences of this sluggishness of ' the bowels were by no means what might have been expected, ' for on comparing a number of cases in which the body had been

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'all along regular, with others in which there had been no ſtool, 'the former did not appear to have been particularly exempt 'from thoſe ſymptoms which might plauſibly have been imputed 'to coſtiveneſs in others.'

"The urine is often obſerved in no reſpect different from that of a perſon in health; at other times it is found pale, high coloured, clear, turbid, without ſediment, or with a great deal, and ſometimes more or leſs tinged with blood; in ſhort, it aſſumes in different patients, or ſometimes in the ſame at different times, all the various appearances obſerved in other fevers.

"There is no excretion of ſuch conſequence in the plague as that by the ſkin. When the ſkin remains parched, or when only ſlight, clammy, partial ſweats appear, the prognoſis is bad; when on the other hand a thin, general, and copious ſweat takes place, it often proves more or leſs critical.

"Dr. Ruſſell obſerves, that the breath and perſpiration were ſeldom or never fetid. Other writers, however, have obſerved, that they are often fetid to a great degree.

"As in many, perhaps in all the other exanthemata, epileptic fits now and then occur. They are a rare ſymptom in the plague. When they appear, they generally precede an eruption.

"Slight convulſive motions of the limbs and ſubſultus tendinum are frequent; with reſpect to other convulſive motions, hiccup rarely, and ſneezing almoſt never, attends the plague.

"Hemorrhagies are a common ſymptom, and unleſs very moderate, generally indicate much danger. They are frequent, as appears from what has been ſaid, from the ſtomach, inteſtines, and kidneys. They are more common however from the noſe, and in women from the uterus, than from other parts. Theſe, particularly the hemorrhagy from the noſe, when they occur early in the diſeaſe, and the patient is young and plethoric, ſometimes bring relief. At a later period, hemorrhagies are always unfavourable, and when they become profuſe, the patient ſeldom recovers. It has been remarked, as might have been inferred a priori, that the blood which flows in theſe hemorrhagies is thinner in proportion as the diſeaſe is further advanced.

"Such are the ſymptoms of the worſt form of the plague. The ſtrength gradually ſinks, till the pulſe impreſſes the finger with only a weak, undulating, or tremulous motion, with frequent intermiſſions. The ſurface, particularly on the extremities, becomes cold and covered with clammy moiſture, the pulſe cannot be felt, and the patient calmly expires, or, as frequently happens in all idiopathic fevers, is carried off by convulſions.

[To be continued.]

MEDICAL

MEDICAL AND PHYSICAL INTELLIGENCE.

[FOREIGN AND DOMESTIC.]

Van Mons, on the Rhus Radicans, or Toxicodendron.

(Extracted from a Memoir in the Actes de la Société de Medicine, Chirurgíe, et Pharmacie, etablie à Bruxelles, Tome I.)

This Memoir is divided into five sections. 1. Botanical description of *Rhus radicans*, 2. On the poison of *Rhus radicans*, its effects on the human body, and the means of preventing them. 3. On the nature of the deleterious exhalations of *Rhus radicans*. 4. Chemical analysis of the plant, in which a peculiar principle seems to exist; on the properties and nature of this principle. 5. On the effects of *Rhus radicans* as a remedy, its specific virtues in palsies and herpetic diseases.

The ancients had but a very imperfect knowledge of the *Rhus radicans*, or *Toxicodendron*, both which species of LINNÆUS ought to be united under one; which opinion Cit. VAN MONS is inclined to adopt on the authority of TOURNEFORT; and he adds a full botanical description of the plant, with a figure of it.

It has been generally asserted, that the venomous part of this plant is lodged in the milky juice which it yields; more accurate observations, however, have shown that the deleterious effects of the *Rhus radicans* originate in a gaseous substance, which is disengaged from the living plant, whereas the dry and withered leaves are quite deprived of it. The effects produced by this plant in the human body differ according to the constitution of the persons that expose themselves to its action; and also according to the circumstances under which it acts on the human body. For it has been remarked, that this gas is almost without any effect during the time when the plant is exposed to the direct rays of the sun; but it becomes very efficacious when it is exhaled during night, in the shade, or on a rainy day; from which phenomenon Cit. Van Mons is induced to think, that the deleterious exhalation is a gasiform juice, imperfectly elaborated by the plant; an idea which seems to be confirmed by the observation that the gas exhaled in sunlight appeared to be nothing but pure oxygen gas, whereas the gas exhaled during night, consisted of hydrogen and carbon; from which circumstance he concludes that the basis of the pernicious gas is retained in the plant, and changed into the substance of the plant by the action of the sun. The effects which are produced by the poison of *Rhus radicans*, consist in a tumour and swelling of the head, a burning sensation or itching in the skin, and in blisters that rise on the skin. The common effect of the poisonous exhalations in

in our climate, is a burning fenfation at the fore-arm and neck, which however difappears in a fhort time. By the chemical analyfis of the plant, the following refults have been obtained: 1. The prevailing fubftance is a peculiar principle, an hydro carbon, which is extremely combuftible, and is contained in the ftalk as well as in the leaves. 2. A great deal of tanin or tanning principle is likewife to be found in the plant. 3. Gallic acid, and but a little colouring matter, notwithftanding the dark green of the leaves. 4. No refin, but fome gum. From this account, it appears with what little reafon the effects of that plant have been formerly attributed to the juice, the nature of which feemed to be unknown.

Cit. DUFRESNOY was the firft who difcovered the great efficacy of the Rhus radicans in palfies and herpetic difeafes, it being never employed before him as a medicine. His obfervations have been confirmed by numerous experiments made by practitioners of different countries.

The beft mode of adminiftering the *Rhus radicans* is undoubtedly in form of an extract, which, as may appear from the above ftatements, is entirely deprived of its noxious particles. The firft time, however, it was employed with much precaution, and the dofes no higher than three pills every day, each confifting of five grains; but it was afterwards obferved that it might be given without injury in the dofes of one ounce a day. DUFRESNOY combines the internal ufe of the plant with the external application of the oil of this plant, with which the affected parts are to be rubbed three times a day. It is made by a hot infufion of the ftalks of Rhus radicans and the hyofciamus (in the proportion of 2 : 6.) in the oil of olives, which is fuffered to ftand fifteen days. The extract may be prepared in five different ways; from the frefh leaves by expreffion, from the dry leaves, from the juice, &c. all which preparations are accurately defcribed in the Memoir. DUFRESNOY gives the preference to the extract made of the frefh leaves, but VAN MONS to that of the dry leaves. This excellent Memoir terminates with the analyfis of a pamphlet of Prof. WURZER on the *Rhus radicans*, in which, feventeen cafes of palfies of the inferior extremities, and of hemiplegies are related, which were cured by the Rhus radicans, to which he adds a cafe of a paralyfis hepatitis, and of a convulfive diforder, in which that plant proved fuccefsful. We conclude this extract with the wifh that this new remedy may ftand the teft of experience and impartial obfervation.

Obfervations on the good Effects of Acetic Ether, applied in Frictions againft Rheumatic Paroxyfms and the Gout by Cit. DESPARANGES.

The effects of acetic ether againft rheumatifm have been confirmed by repeated obfervations, efpecially by Cit. MARTIN, whofe remarks we communicated in a former number of this Journal.

We now beg leave to add two obfervations of Cit. Desparanges; tending to prove the fuccefsful application of that remedy in a cafe of gout, and in another of rheumatifm.

Obs. I.—Cit Blondeau, fifty years of age, of a bilious temperament, had been afflicted three years with a violent lumbago, that attacked him particularly in the fpring and autumn. In the beginning of May, 1799, he was feized with the moft violent pains, for which feveral remedies were employed in vain for three days. At laft he had recourfe to the acetic ether, with which the frictions were made in the dofe of three drachms. In a fhort time his fufferings confiderably abated; at the end of five frictions the patient was again able to walk, and felt but little fenfation of pain, and fince that time he has never fuffered a new paroxyfm.

Obs. II.—Mrs. Chartran, aged 51, of a fpare habit, and dark complexion, had been fuffering for two years violent ifchiadic pains, which increafed to fuch a degree that they were almoft infupportable. Cit. Desparanges was called to attend her; he found an inflammation and fwelling at the articulation of the thigh with the trunk, the reft of the thigh being neither fwelled nor inflamed, but only painful. The patient was without fever. Under thefe circumftances, Cit. Desparanges did not think it proper to delay the application of acetic ether in frictions. He began with two drachms, which however had no effect the two firft times, but the dofe being increafed at the third time to half an ounce in the morning and evening, the pains became lefs violent, and the patient had a copious topical perfpiration. This treatment being continued fix days, a general perfpiration enfued, after which the patient could rife a little; on the tenth the pains difappeared almoft entirely, and on the twelfth the cure was complete.

Cit. Chaussier's *Method of preserving Anatomical Preparations.*

Citizen Chaussier has communicated to the Society of Medicine at Paris, a new method of preferving animal fubftances. After having enumerated the different methods employed for that purpofe, he points out their defects and infufficiency. For preparing parchment, the fkins are macerated in water, difengaging from them the unctuous particles, and diffolving likewife a part of the mucilage, fo that nothing remains but the fibrous part. For tanning, the hides are put into lime-water, fulphuric acid, or into a folution of alkali, and afterwards expofed to the action of the *tanin* or tanning principle. For preferving anatomical praparations, they are generally put into alkohol, which dries them up and entirely changes their proper form; and though they are in this way preferved from putrefaction, yet they do not remain untouched by the infects. The carbonate of foda, and the fulphat of iron, which are alfo ufed for keeping off putrefaction from animal fubftances, render them foluble in water by combining themfelves with the unctuous particles, and forming a foap with them, whereby the fize and form

form of the preparations are confiderably altered. In order to avoid all these inconveniencies, Cit. CHAUSSIER suffers the parts intended for prefervation to be macerated during a longer or shorter time, from three to eight or ten days, according to their refpective fize, in a folution of oxygenated muriat of mercury in diftilled water. This liquor being always kept in a perfect ftate of faturation, by adding from time to time frefh muriat of mercury for that which is decompofed, imparts a great folidity to the parts impregnated with it, by giving confiftency to the gelatinous parts without changing their fize and form, and when expofed to the air for fome time, they are fecure from corruption and infects. It is through the medium of this mode of preparation, that Cit. CHAUSSIER has made feveral interefting obfervations on the ftructure of the brain, and particularly of the fpinal marrow, for he difcovered that this part, after being deprived of its *pia mater*, is compofed of fix very diftinct bundles; farther, that all the nerves which arife from this part of the brain are by no means fimple productions of its fibres, but that they are inferted in it like hairs, by means of bulbs which adhere to the medulla by feveral fmall roots; and when thefe nerves are pulled out, a double row of fmall regular holes will appear to the eye, into which the bulbs are implanted. A portion of brain, prefented to the Society by the inventor of this method, had the folidity of wood, without the leaft change in its natural fize and form; another brain and fpinal marrow prepared in the fame way, very diftinctly fhowed the holes into which the nerves were ingrafted. The celebrated RUYSH made alfo ufe of a liquor and of injections to preferve his excellent anatomical preparations, by means of which he had fucceeded in preferving the body of his own daughter, in the colour of life and frefhnefs of youth. This liquor, which he always kept as a myftery, feems to be the fame with that of Cit. CHAUSSIER, or at leaft fomething analogous. According to RUYSH, it likewife made the gelatina folid, and by degrees as hard as wood; the albuminous matter coagulated by it, and the cryftalline lens put into it, became opacous and white; the brain obtained in it a cafeous and folid confiftency. For colouring the injections, it is advifable to take madder or cinnabar, but never to make them of wax, or any other unctuous matter, but of a mucilaginous folution, as the folution of *ichthyocolla*, or ifinglafs. After having injected the parts, they ought to be put into the above folution of oxygenated muriat of mercury, where the matter for injecting concretes, and becomes folid. For preferving whole bodies, it is neceffary to make openings into the great cavities, head, cheft, and belly, large enough for the liquor to penetrate into them, as without this precaution the inteftines will not be fecured from corruption. RUYSH himfelf always made fuch incifions for the above purpofe.

Prof. HIMLY, of Brunfwick, has prefented a Memoir to the Royal Society of Sciences of Gottingen, in which he communicates fome interefting remarks on the paralyfis of the pupil, occafioned

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by the topical application of *extractum hyosciami* on the eye, similar to that which has been observed from the use of *Belladonna*, (Medical and Physical Journal, vol. iii. p. 367,) and the *aqua Laurocerasi*. The first observation was made in a very obstinate case of weakness of sight, where a known remedy, of *one scruple* of extractum hyosciami dissolved in *one* ounce of distilled water, had been applied on the margins of the eye-lids, after which, a most surprising dilatation of the pupil ensued, that lasted for a considerable time. This experiment has been repeated by Prof. HIMLY on more than twenty persons, by letting fall a few drops of this solution into the eye, in which it ought to be kept for a short time by bending the head backward. It never failed to produce the intended effect, if the pupil itself had any power of motion left. The great dilatation of the pupil appears one or two hours after the application of the extract, and generally lasts for five or six hours, without in the least affecting the retina. The water distilled from the fresh herb of hyosciamus had little or no effect, and likewise the *emplastrum de hyosciamo*, or *de belladonna*. The author adds a few remarks on the use that is likely to result from the above experiment, which are the more interesting as his own experience has furnished him with observations, where it proved of some service. According to them, it may be employed as a diagnostic expedient in the cataract, in order to discover whether the capsule adheres to the iris or not, whether the red spots, which in some cases appear before the eyes of such patients, originate from a vitium retinæ, or from a peculiar red coloured obscuration of the crystalline lens. It may also serve as a palliative remedy in the cataract, when the crystalline lens is only darkened in some places. The author has accurately determined the cases in which that expedient seems to prove advantageous; and he intends treating this subject more copiously in a future publication.

Mr. KARSTEN, of Berlin, has repeated the experiments of BOUILLON LA GRANGE on the analysis of cork, and on the peculiar acid that is contained in it, (Medical and Physical Journal, vol. iii. p. 89,) whose principal character consists in dissolving with great difficulty in water, on which account it can be only obtained in form of a powder. The acid, however, which Mr. KARSTEN extracted from cork, shoots into crystals that are freely soluble in water. From the other characters which this acid shewed, it appeared to be nothing but oxalic acid. Mr. KARSTEN, however, is inclined to ascribe this difference of results to the difference of cork that was respectively used for the analysis.

Mr. NŒF, surgeon of Wermgerode, relates two cases of carious ulcers, in which he employed the juice of *plantago angustifolia* with the greatest success, the use of which remedy is also much recommended by Prof. ARNEMAN, who found it likewise effectual in his own practice.

Cit.

Cit. COQUART'S *Observations on some Effects of Lightning and Electricity in Gonorrhœa.*

OBSERVATION I.—Towards the end of July, 1800, Cit. N. aged forty, of a very irritable constitution, had a gonorrhœa during fifteen days, attended in the beginning with inflammatory symptoms. The patient, however, was so far recovered by a proper treatment, that the pains, as well as the running, considerably abated. One night when he was sleeping on his bed, almost naked, and with the windows open, he was suddenly awakened by the noise of thunder; he leaped from his bed, and having walked in the chamber for some minutes, he went to bed again. He, however, immediately felt some pains in the perinæum, and had a restless night: The next morning he had a complete suppression of urine, and all his attempts to make water produced nothing but a few drops of blood; the running had likewise ceased. The patient was immediately ordered to be put into a bath, and he then made a little water mixed with blood. About mid-day another bath was used, after which the urine proceeded in small streams, and bloody. When he had taken a third bath in the evening, no more blood came off with the urine; and by a strict regimen, the natural flow of the urine was re-established. The patient went afterwards into the country, and having discontinued the baths, the gonorrhœa spontaneously returned; the matter was milky, and not copious; the pains in the perinæum were slight, and only felt at the time of erection. Some time after, the running, and all the other symptoms of the gonorrhœa disappeared entirely. This observation naturally leads to the question, whether the suppression of urine is to be attributed to the surprise of the patient, by being so suddenly awakened by the noise of thunder, or to an electric action? However, this effect of electricity is not quite so much proved as in the following observation.

OBSERVATION II.—An Englishman, who had for the space of two years, a running from the urethra, against which he had consulted the best practitioners in France and England, without the least success, applied at last to Citizen ROUELLE, physician at Loeven, of great reputation. This gentleman having again tried all possible internal and external remedies to no purpose, had recourse to electricity, with which he used to amuse himself. Having, therefore, introduced into the urinary canal of the patient an iron thread, he drew out of it only one electric spark, whereupon the patient instantly felt a vehement pain in the perinæum; the gonorrhœa, however, completly disappeared, and the patient was perfectly cured.

A memoir has lately been addressed to the Medical Society at Paris, containing a very singular case of mental derangement. The subject is a man, sixty-two years of age, who, in consequence of an hemiplegia, with which he was twice attacked, and of which he was cured by a proper treatment, had lost his memory entirely, so as not to recollect his own name, nor that of

his

his wife and children; and only retained the expreſſions *oui*, yes; *non*, no; *beaucoup*, much; *très bien*, very well; *au charme*, charmingly; *point de tout*, not at all; *c'eſt vrai*, it is true; *c'eſt juſte*, it is right; *á merveille*, &c. The phyſician who conſults the ſociety, deſires to know, Whether there exiſts a phyſical or moral treatment, by which this patient may recover his memory? The obſervation being communicated to the *Societé Philomatique*, the Citizens HALLE and MOREAU have been appointed to give an account of this ſingular caſe.

Citizen PENON has preſented three memoirs to the National Inſtitute, containing ſome obſervations on the uterus, in which he particularly examines the ſucceſſive dilatations of that organ, and of its *cervix*, during the periods of pregnancy, eſpecially in the eighth month. The theory, however, which he attempts to eſtabliſh, according to his anatomical views, does not at all ſeem to agree with the principles of modern and ſound phyſiology, which is, for the honour of our times, raiſed on the ruins of the pure mechanic ſyſtem. In order to enable our readers to judge of what we have aſſerted, we ſhall add here, a fragment of the extract of the above memoirs. Cit. Penon is of opinion, that the aqueous particles, and even the urine, dilute the uterus, by acting on it as emollients; and he thinks it is, for this reaſon, that Nature has placed the urinary bladder and the uterus ſo near to one another. He likewiſe thinks, that the membranous burſa, furniſhed to the uterus by the peritonæum, does not penetrate between this and the bladder, the laſt organ being intended to contribute to the dilatation of the uterus during the time of pregnancy, for which purpoſe it ought to be contiguous with the uterus, into which it lets paſs, through its pores, a part of its liquor, for mollifying it, which, he imagines, will be greatly ſupported by injecting water into the uterus. Having obſerved that acids impart a very great elaſticity to the ſubſtance of the uterus, he is led to think, that the milk, which is depoſited after delivery into the cellulæ of theſe organs, may contribute to the contraction of the uterus by becoming ſour. Such are the groſs ideas of the author, which we have merely communicated for the purpoſe of ſhowing how unacquainted ſome people are with the improvements of our times, as the above theory would ſcarcely have done honour to the darker ages of ſcience. It is ſtill more to be wondered, that theſe reſults of a miſtaken imagination, were admitted before ſo reſpectable a body as the National Inſtitute of France, which is occupied in ſpreading true knowledge and enlightened principles.

Cit. LASSUS imagines, that the hernia umbilicalis originates, in many caſes, from the too great ſize of the liver, which ſometimes increaſes in an extraordinary manner from a particular diſpoſition. This morbid conformation terminates, in moſt caſes, fatally.

Amongſt

Amongſt the intereſting contents of the *Journal de Phyſique*, the memoir of Cit. Cuvier has particularly engaged our attention. The author remarks, that notwithſtanding the moſt aſſiduous reſearches, we have not yet ſucceeded in uniting the different phenomena of voice under a certain theory, a circumſtance which, he thinks, is partly owing to anatomiſts not being ſufficiently acquainted with the nature of muſical inſtruments, partly to their having merely confined their inquiries to the human voice, the moſt complicated of all, and leaſt ſimilar to the inſtruments of muſic which we know. In order, therefore, to proceed from the ſimple to the compound, and by that means to find out, by induction, new reſults, Cit. Cuvier has thought it proper to ſtudy the organ of voice in birds, and afterwards to make an attempt at explaining the human voice. His memoir may be reduced to the following articles:

1. *On the researches of other anatomists concerning the voice of birds;* where he gives an account of the works of Caſſerius, Perrault, Heriſſant, Vicq. d'Azyr, Bloch, and of his own ideas, publiſhed in one of the numbers of the *Magasin Encyclopedique.*

2. *On the place where the voice of birds is formed:* In which article he ſtates, that for producing ſounds by means of a wind inſtrument of muſic, the intervention of an elaſtic body is neceſſary, that is put into vibration, and that the tube only modifies the ſound produced at its mouth by the ſonorous body which is put into vibration. In man, and other animals of the mammiferous tribe, the mouth and naſal cavities are the only parts that may be compared with the tube of an inſtrument, the ſound forming itſelf at the end, and in the upper part of the trachea. The conſtruction of the vocal inſtruments in birds differs in a great meaſure from this, becauſe their trachea, or wind-pipe, modifies the ſound which is formed at its lower extremity, at which place a true glottis exiſts. This obſervation is confirmed by three experiments, according to which, a black-bird, a magpye, and a duck, continued to cry, notwithſtanding their throats being cut through, and the duck even gave ſounds after the complete amputation of the neck.

3. *General ideas on the means by which the birds vary the sound.* They conſiſt in the variation of the glottis in the *larynx inferior*, and of the wind-pipe, and in the faculty of contracting and dilatating the ſuperior glottis.

4. *On the larynx inferior.*

5. *On the wind-pipe.*

6. *On the larynx superior.*

Theſe articles are devoted to more detailed reſearches of comparative anatomy, and we are hence entitled to conclude, that the organ of voice in birds is a true inſtrument of wind muſic, of the claſs of trumpets, and particularly of the ſackbuts, (tombon). Cit. Cuvier announces, at the end of this memoir, that he intends to apply his doctrine for explaining the organs of voice in man, and the mammiferous animals.

Cit.

Cit. Demanet is inclined to aſcribe, in a great meaſure, the cauſe of convulſions that affect women during the period of pregnancy and delivery to an *anaſarca*. The moſt reſpectable accoucheurs have only adopted three cauſes of convulſions in pregnant women; loſs of blood, its too great abundance, and the pains of parturition. Cit. Demanet, however, relates ſix caſes, in which he pretends to have obſerved the above cauſe; it muſt, however, be allowed, that it is difficult to conceive in what manner the convulſions can be occaſioned by this ſymptom.

Mr. Mursinna, of Berlin, has made ſome remarks reſpecting the cure of tetanus, or locked jaw, a diſeaſe, the aitiology of which is by no means aſcertained. According to the experience of this great ſurgeon, *opium* ſtill remains the moſt efficacious and powerful remedy againſt that dreadful affection. It ought to be given at the firſt appearance of the diſeaſe, in ſmall doſes, and repeatedly adminiſtered at ſhort intervals. A bath of warm water with a large quantity of ſoap added to it, greatly advances the cure; and it has been obſerved that the patients receive in the bath the power of opening the mouth, and of ſwallowing. Opium, however, ought to be given in a liquid form; on which account, according to Mr. Murſinna's experience, the laudanum Sydenhami is moſt preferable, as the patient can take it in ſufficiently large doſes without perceiving any nauſea. There is nothing to fear from too great a doſe of opium, becauſe it muſt be given in increaſed doſes, till the diſeaſe begins to abate; and at this period only, the doſes may be leſſened. In combination with the internal uſe of opium, bliſters, frictions with quickſilver, and opium, may be applied, by which the effect of that remedy is greatly ſupported. Mr. Murſinna employed in two caſes, the method of cure propoſed by Dr. Stutz, but without ſucceſs. The firſt was a triſmus and tetanus, which enſued a caſtration, eight days after the operation had been performed. The patient died in three days from the beginning of the diſeaſe. In another caſe the triſmus ſupervened twelve days after the extirpation of a ſcirrhus, and though the ſpaſm ſeemed at firſt to abate, and even to diſappear by that method of cure, yet it came on again unexpectedly, and put an end to the life of the patient thirty hours after its firſt appearance.

It is with pleaſure we obſerve the rapid progreſs of the Vaccine Inoculation on the continent. There is hardly any conſiderable place in Germany where it is not practiſed, and there exiſts a general emulation among phyſicians, particularly of that country, to propagate this great diſcovery, by which the laſt century ſeems, as it were, to compenſate the bloodſhed and the loſs that the preſent unhappy war has inflicted on Europe, as it offers to us the conſoling hope of extirpating the Small-pox, that ſcourge of mankind,

kind, which for ſo many centuries has afflicted humanity in its moſt tender parts. It is particularly with this great view, that ſeveral phyſicians, among whom we find the venerable name of Hufeland, have thought it worthy their attention to take meaſures, in order to ſpread the inoculation of the Cow-pox. The phyſicians of Geneva have publiſhed the following advertiſement, which, in that important moment when the children are chriſtened, is delivered by the clergyman to the godfathers, after the baptiſm is over, in order to make the people more inclined for the Cow-pox, by preſenting to them the Vaccine Inoculation as a matter of conſcience:—"The child that is juſt chriſtened is, amongſt different other dangers, expoſed to become a ſacrifice to the Small-pox, a diſeaſe that, ſince the eighth century, has ſpread itſelf through Europe in ſuch a manner, that it is morally impoſſible to preſerve the child from it, except by inoculation. Luckily, and by a particular favor of Providence, a diſcovery has been lately made, confirmed by the teſt of experience, which is both harmleſs and ſecure, and may be employed in all ſeaſons, in weak, tender, and new-born children; a remedy that is never attended with any bad ſymptom, but of the mildeſt nature, viz. the Vaccine Diſeaſe, an affection that has hitherto proved ſlight, and very mild, and affords the ineſtimable advantage of not being contagious, ſo that it may be inoculated in a houſe without any fear of danger to the neighbours, though it, at the ſame time, averts for ever the misfortune of receiving the Small-pox. If you, Parents! wiſh to defend this child from that danger, we conjure you to get it immediately inoculated with the Cow-pox! Haſten to preſerve it from that dangerous diſeaſe, which daily fills your hearts with deſolation, and by which your children riſk every moment to be infected; do not even deliver them to the nurſes, before you have reſcued them from this danger. The experience and obſervation of the phyſicians and ſurgeons of this place, who know themſelves the feelings of a father, and who have inoculated their own children, ought to encourage you to follow their example. In the name of humanity, by all that is dear to you, we requeſt you to follow their example; if not, we juſtify ourſelves, when once you ſhed tears for the loſs of this child: You have not been willing to avail yourſelves of this remedy that was offered to you, ſimple and eaſy as it is; you have refuſed it; you have unreaſonably doubted to apply it in time. We, the ſubſcribed phyſicians and ſurgeons, have made it our duty to inoculate every child that has been brought to us, without the leaſt recompence. Our fellow citizens know, that we have hitherto accepted no reward from any perſon whoſe circumſtances did not allow it."

Signed, *Vieuſſaux, Odier, Vignier, Monget, Veillard, Coindet, De la Rive, Peſchier,* phyſicians; *Jurine, Fine, Maunoir,* ſurgeons.

The celebrated French mineralogiſt, Hauy, is about to publiſh a work on mineralogy, conſiſting of three volumes, in large octavo. From the abilities of this literatiſt, we have reaſon to ex-

pect, that it will become one of the moſt elaborate and inſtructive works we poſſeſs on that branch of natural ſcience.

A third volume of MEDICINA NAUTICA is nearly ready for publication, which, for original matter and more careful ſelection, will ſurpaſs the preceding volumes. The medical department, as well as others in the navy, acquired freſh activity from the flag of Lord St. Vincent appearing in the Channel; new facts and obſervations have been recorded to more advantage; and as the preſent volume is likely to bring down the occurrences of the fleet to the concluſion of the war, the whole will exhibit a wider field of medical-practice in the navy than has ever been reaped by others.—Communications for this work may be addreſſed to the author, *post paid*.

To the EDITORS of the MEDICAL and PHYSICAL JOURNAL.

Gentlemen,

WE beg leave to inform you of a newly invented Truss, of which we are the patentees, and which we are assured, by several professional gentlemen of the first respectability, far exceeds any other which has hitherto been invented, both for durability and effect. Its spring being much stronger and more highly tempered than any other, it is capable of being more exactly fitted to the parts on which it is to act, and of resisting any force or exertion by which it might be liable to be misplaced; nor is its influence to be weakened by long use.

The annexed letter we received from the surgeon of the Greenwich Hospital, and we have since had the honour of being appointed by the honourable board to supply that institution. We have also the appointment to the Chelsea Hospital, beside other appointments to workhouses and charities, and also the universal approbation of every individual who has been recommended to us by the Faculty. We are persuaded the more this invention is known, the more it will be prefered for its peculiar advantages over every other hitherto offered to the public.

We are, &c.

Charing Cross, Aug. 19, 1801. TATHAM and EGG.

P. S. Sold only at Tatham and Egg's, gun makers, No. 37, Charing Cross.

Gentlemen, *Royal Infirmary, June* 24, 1801.

AGREEABLY to your request I herewith send you the result of my trial, of two months, of your elastic trusses; their durability and security are certainly superior to any thing of the kind, I have ever met with.

On

On the first application of them, a tightness is complained of, common to all trusses, but they grow easier by use, and I do not find that straps are absolutely necessary to them; and, therefore, it is my opinion, that when the consideration of the above qualities is added to that of their very moderate price, it must be an object to any chirurgical institution to give them the preference. I am, &c.

W. MILLER.

To Meſſrs. Tatham and Egg, Gun Makers,
No. 37, Charing Cross, London.

To the EDITORS of the MEDICAL and PHYSICAL JOURNAL.

Gentlemen,

THROUGH the medium of your very useful Publication, I beg leave to make known to the medical world, as early as possible, a newly invented Elastic Spring Truss, for the relief of those that are ruptured; and should I obtain an admission of it into your Journal, I shall ever esteem myself,

Your's, &c.

J. WHITFORD, Surgeons Instrument Maker.

St. Bartholemew's Hospital, Aug. 17, 1801.

The occasional difficulties and dangers attending those persons afflicted with ruptures, for the want of a proper truss or bandage, and the bad effects which frequently occur in the application of common trusses not fitting the part affected, call loudly for some improvement. I have for several years past spent much time upon this kind of mechanism, and about three years ago I made an improvement upon the common truss, by lengthening the pad, so as to cause the spring, surrounding the body, to apply higher upon the hips of the patient, and the pad to come lower, which prevent the truss from slipping off the part, when once properly applied. This advantage has given very great satisfaction; but I thought that some improvement was still wanting, to prevent the wearing of under straps; and I have great satisfaction in informing the public, that I have at last succeeded in inventing a kind of truss, that, by the means of a spring in the pad, which makes a constant pressure, and that pressure to any degree requisite against the opening, so that the intestine is kept up, the opening in the tendon contracted, and the sides of the sac made to approach each other, so that a radical cure, in many instances, will be effected. The mechanism of the pad supersedes the necessity of under straps, which are inconvenient and galling to those who use much exercise, whereas this truss is perfectly easy to the wearer, and adapts itself to any attitude in which the body may be placed.

J. Whitford has the satisfaction of knowing, that his invention is

is sanctioned by the opinion and recommendation of several of the Faculty, and by those who have most feelingly expressed their approbation, from experiencing its efficacy, in the comfort, ease, and safety obtained by it.

The inventor cannot suppress the pleasure of adding, that he is extremely happy in the reflection, that he has discovered an apparatus for the relief of a disease, which, if neglected, frequently proves the most painful and dangerous to which the human frame is liable.

LECTURES.

Medical School of St. Thomas's and Guy's Hospitals.

The Winter Course of Lectures at these united Hospitals will commence in the following order.

Anatomy and Surgery, by Mr. CLINE and Mr. A. COOPER, on Thursday, October 1, at one o'clock.

Practice of Medicine, by Dr. BABINGTON, on Friday, Oct. 2, at ten o'clock in the morning.

Midwifery and Diseases of Women and Children, by Dr. LOWDER and Dr. HAIGHTON, on Saturday, Oct. 3, at eight in the morning.

Chemistry and Experimental Philosophy, by Dr. BABINGTON and the Rev. Mr. ROBERTS, on the same morning, at ten.

Physiology, or Laws of the Animal Œconomy, by Dr. HAIGHTON, on Monday, Oct. 5, at a quarter before seven in the evening.

Theory of Medicine and Materia Medica, by Dr. CURRY, on Tuesday, Oct. 6, at seven in the evening.

Principles and Practice of Surgery, (illustrated by select cases under his care in the Hospital) by Mr. ASTLEY COOPER, on Monday, Oct. 12, at eight in the evening.

In addition to these, Dr. SAUNDERS will, early in October, begin a Course of Clinical Lectures on select Medical Cases under his care in the Hospital.

The Lectures given at these Hospitals are so arranged, that no two of them interfere with each other in the hours of attendance; and the whole is calculated to form a complete circle of Medical Instruction.

N. B. Mr. FOX, in the Course of the Season, will deliver his Lectures upon the Structure and Diseases of the Teeth.

Terms and other particulars to be had at the Hospitals, or of Mr. Cox, bookseller, St. Thomas's Street, Southwark.

Medical Theatre, St. Bartholomew's Hospital.

The following Courses of Lectures will be delivered at this Hospital during the ensuing season.

On the Theory and Practice of Medicine, by Dr. ROBERTS.

On

On Anatomy and Physiology, by Mr. ABERNETHY.
On Comparative Anatomy, by Mr. MACARTNEY.
On the Theory and Practice of Surgery, by Mr. ABERNETHY.
On Chemistry and the Materia Medica, by Dr. POWELL.
On Midwifery and the Diseases of Women and Children, by Dr. THYNNE.

Medical Theatre, London Hospital.

The Lectures on Anatomy, Physiology, and the Principles and Operations of Surgery, with practical Anatomy, as usual, will commence on the 1st of October, by Mr. BLIZARD, Mr. J. BLIZARD, and Mr. HEADINGTON, Surgeons of the Hospital.

The Medical Lectures in the *University of Glasgow* will begin on Tuesday the 3d of November, at the following hours.

Dietetics, Materia Medica, and Pharmacy, by Dr. MILLAR, at ten o'clock in the forenoon.

Midwifery, by Mr. TOWERS, at eleven o'clock.

Theory and Practice of Physic, by Dr. FREER, at twelve.

Anatomy and Surgery, by Dr. JEFFRAY, at two o'clock in the afternoon.

Chemistry and Chemical Pharmacy, by Dr. CLEGHORN, at seven o'clock.

Clinical Lectures on the cases of patients in the Royal Infirmary, by Dr. FREER and Dr. CLEGHORN. The first Lecture by Dr. Freer, on Thursday, the 12th of November, at six o'clock in the evening.

Dr. BROWN will begin his Lectures on Botany about the beginning of May next.

Lectures at the St. Mary-le-Bone Infirmary.

On the Principles of Medical Science, by Dr. ROWLEY.
On the Practice of Physic and Morbid Anatomy, by Dr. HOOPER.

Dr. CRICHTON will commence his Autumnal Course of Lectures on Medicine and Chemistry, at his Lecture Room in Clifford-street, Bond-street, on Monday, October 12. The Lectures on the Theory and Practice of Physic will be delivered every morning, Sundays excepted, at eight o'clock; those on Chemistry every Monday, Wednesday, and Friday, at nine; and those on Materia Medica every Tuesday, Thursday, and Saturday, at the same hour. Further particulars may be obtained by applying to Dr. C. at his house, No. 15, Clifford-street.

Dr. BRADLEY will commence his Autumnal Course of Lectures on the Theory and Practice of Medicine, on Monday, Oct. 5, at the Lecture Room, No. 102, Leadenhall-street, at six in the afternoon. The Course will consist of about Seventy Lectures. Terms, Three

Three Guineas. Private Courses, for the accommodation of young Surgeons in the Navy or Army, are continued every morning at Dr. Bradley's, Delahay-street, Westminster.

A Course of Lectures will be delivered on one or two evenings in the week, during the season, on the Principles of Chemistry and their application to Pharmaceutical Processes, by a Practical Chemist.

Dr. GARNET intends to deliver Lectures on the Theory and Practice of Medicine, and on the Animal Œconomy; he also proposes to deliver Lectures on Chemistry, and those branches of Natural Philosophy particularly connected with Medicine. These different Courses will commence in October next, at his house, No. 51, Great Marlborough Street.

Dr. OSBORN's and Dr. CLARKE's Lectures upon the Principles and Practice of Midwifery, and the Diseases of Women and Children, will begin on Friday, October 2. They will be given in future only at Dr. Clarke's house in New Burlington-street, from half past ten to half past eleven in the morning, for the convenience of gentlemen attending the different Hospitals.

Dr. BATTY's Lectures on Midwifery, and on the Diseases of Women and Children, will commence at his house in Marlborough Street, on Monday, October 5, at eleven o'clock in the morning.

Dr. DENNISON and Dr. SQUIRE, Physicians and Men-Midwives to the Lying-In Charity for delivering poor Women at their own habitations, will begin a Course of Lectures on Thursday, October the 1st, on the Theory and Practice of Midwifery and the Diseases of Women and Children.—In addition to a constant supply of labours for practical improvement, gentlemen will have frequent opportunities of visiting patients in disorders during pregnancy, and the advantage of seeing the treatment of diseases incident to the state of child-birth and early infancy.

Mr. PEARSON, Surgeon of the Lock Hospital and Asylum, and of the Public Dispensary, will begin his Autumnal Course of Lectures on the Principles and Practice of Surgery, on Monday, October 5, at seven o'clock in the evening.—In this Course he will deliver an extensive account of the history and treatment of Scrofula and Lues Venerea.

On the 5th day of October, Mr. T. POLE will commence his Course of Lectures on the Theory and Practice of Midwifery, including the Diseases of Women and Children, at his house, No. 102, Leadenhall-street, near the Royal Exchange, where printed particulars may be had.

Mr.

Mr. Chevalier, Surgeon to the Westminster General Dispensary, will commence his Autumnal Course of Lectures on the Principles and Operations of Surgery, on Monday the 5th of October, at seven o'clock in the evening, at his house, No. 20, South Audley-street, Grosvenor-square, where a printed Plan of the Course may be had, or at the Dispensary in Gerard-street, Soho.

Mr. H. Leigh Thomas will commence his Winter Course of Lectures on the Principles and Practice of Surgery, on Monday, October 5; in which the different operations will be performed, and the medical treatment fully explained.—Further particulars may be known at his house in Leicester-square, or at the Anatomical Theatre, Windmill street.

At the Theatre of Anatomy, Great Windmill-street, Two Courses of Lectures are read by Mr. Wilson during the Winter and Spring Seasons; one Course beginning on the 1st Day of October, and terminating on the 18th day of January; the other Course beginning on the 19th day of January, and terminating towards the middle of May.

In each Course is explained the structure of every part of the human Body, so as to exhibit a complete view of its Anatomy, as far as it has been hitherto investigated; to which are added, its Physiology and Pathology; after which follow Lectures on the Operations of Surgery; and the Course concludes with the Anatomy of the Gravid Uterus.

A Lecture is given daily from two until four o'clock in the afternoon.

A room is likewise open for Dissections, under the direction of Mr. Wilson, and Mr. Thomas, from nine o'clock in the morning till two in the afternoon, from the 10th day of October till the 20th of April; where regular and full demonstrations of the Parts are given; where the different cases in Surgery are explained, the methods of Operating shewn on the dead Body; and where also the various arts of injecting and making Preparations are taught.

Further particulars may be known by applying to Mr. Wilson, at the Anatomical Theatre; or at his house in Argyll-street, Hanover Square; or to Mr. Thomas, Leicester Square.

Mr. Carpue will commence his Anatomical Lectures, at his Theatre, Broad Street, Golden Square, on Monday, the 5th of October, 1801. The Structure of the human Body will be explained, as also its Physiology. The Operations of Surgery will be shown on the dead body. Mr. C's plan prevents him from taking more than ten Pupils. Every day some part of the human body is demonstrated to the pupils, who are required to demonstrate it afterwards to him. Myology is not taught till the pupils are perfectly instructed in Osteology, &c. &c. A general examination takes place every tenth day; and if the pupils do not perfectly

recollect any of the parts that have been lectured on, they are again demonstrated. The Dissecting Room will be open from nine o'clock in the morning till half past two. The pupils are here taught the method of operating, as also the art of making preparations.

Further particulars may be known by applying to Mr. Carpue, at his house, No. 44, Leicester Square.

ERRATUM.

In Dr. Beddoes's Letter, in our last Number, p. 165, *before* occasioned, *insert* has not.

To the PURCHASERS of the MEDICAL and PHYSICAL JOURNAL.

NOTWITHSTANDING every periodical Publication has been raised in its price since the late DOUBLE DUTY *upon paper, and the advance of* 30 *per cent. in the expence of printing, the Medical and Physical Journal has hitherto been continued at its original price. The Proprietor had a stock of paper in his possession at the old duty, and he was unwilling to advance this work till compelled to purchase a fresh supply of paper at the new prices.*

The advance in the paper upon which this work is printed, proves to be FIFTEEN SHILLINGS *per ream, creating an additional expence to the Proprietor, in this single article, of upwards of twenty pounds per month; he is compelled therefore by necessity to add* SIX PENCE *to the price of the Work, and in future to sell all the Numbers at* HALF A CROWN. *Nothing has given him more pain than to be obliged thus to advance the price of the Work; he hopes, however, that the reasonableness of this measure will be apparent, and that no professional man will deprive himself of the valuable information disseminated in* THE MEDICAL AND PHYSICAL JOURNAL, *by the present unavoidable advance in its price.*

Persons who reside abroad, and who wish to be supplied with this Work every month as published, may have it sent to them, FREE OF POSTAGE, *to New York, Halifax, Quebec, and every part of the West Indies, at three guineas per annum, by Mr.* THORNHILL, *of the General Post Office, at No.* 21, *Sherborne Lane; to Hamburgh, Lisbon, Gibraltar, and every part of the Mediterranean, at three guineas per annum, by Mr.* BISHOP, *of the General Post Office, at No.* 22, *Sherborne Lane; to the Cape of Good Hope, or to any part of the East Indies, at two guineas per annum, by Mr.* GUY, *of the India House; and to any part of Ireland, at two pounds ten shillings per annum, by Mr.* SMITH, *of the General Post Office, at No.* 3, *in Sherborne Lane. It may also be had of all persons who deal in Books in every part of the world.*

An extraordinary Tumour of the Eye.

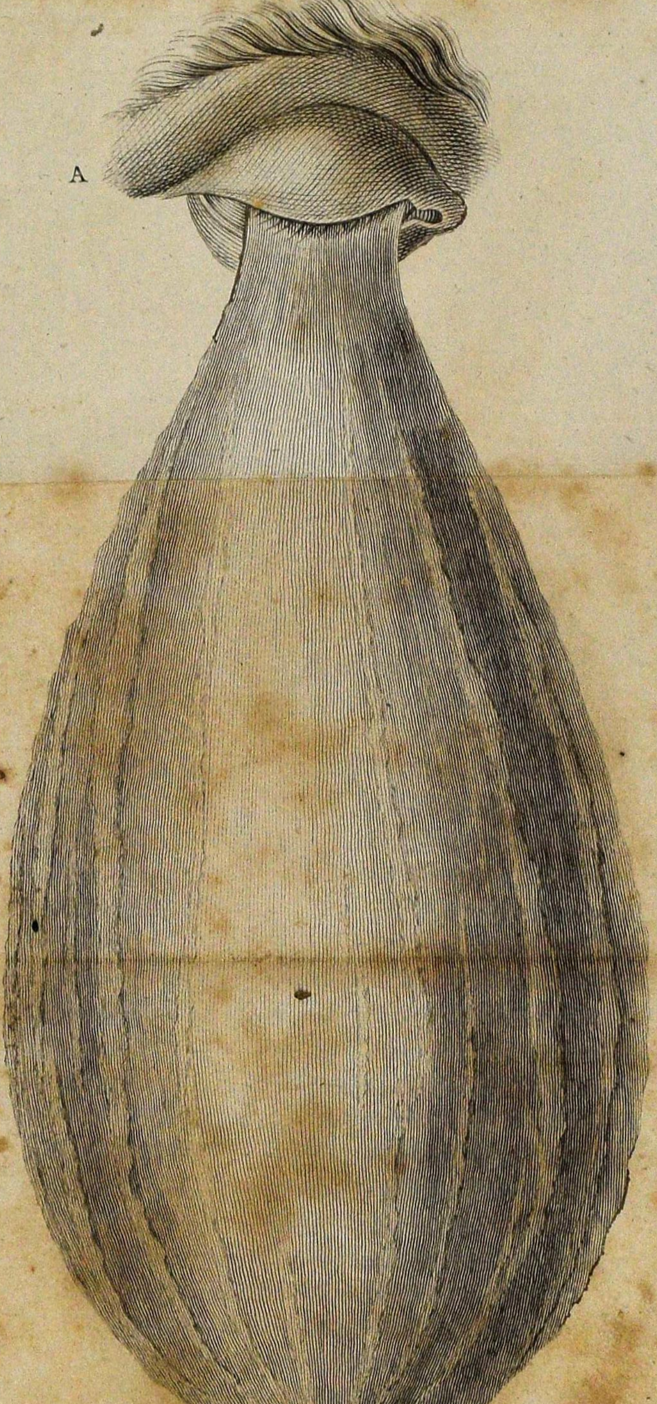

Plate I. The appearance of the Eye previously to the operation.

Printed for R. Phillips, No. 71, St. Paul's Church Yard.